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The Study of Behavior

We must, to change the Greek epigram a little, ASCEND DOWNWARD *and* DESCEND UPWARD, *if we would reach truth, or any true persuasion of it.*

The Study of Behavior

Q-Technique and Its Methodology

By

William Stephenson

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IN REMEMBRANCE OF

CHARLES SPEARMAN · F.R.S.

AND

WILLIAM BROWN · M.D.

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PART I

BASIC FORMULATIONS

IT WAS a Cambridge psychologist who called a well-known Chicago philosopher "and his bunch" the "do-nothing" methodologists . . . "who sit around and philosophize when there is so much crucial work to be done." The problems of methods and definitions, he continued, should be taken up after the fields have been explored, the problem solved, and the data safely gathered in. The reverse, we are sure, is true. Only with the proper methodological principles can any problems be solved and any data safely brought into the scientific field. This, in any case, is to be the standpoint of this book. It is not unsound to say, we believe, that neglect of methodological principles has resulted, in psychology, in a vast fumbling about among facts, with no rhyme and little reason. We are to propose, instead, that the science of behavior can be immeasurably improved by attending to a few principles upon which we have based the method now well known as "Q-technique."

Our concern, however, is not to be with Q-technique alone, or even principally. Rather, it is with a challenge to psychology, in certain of its aspects, to put its house in scientific order. We are to consider a *methodology* to serve this purpose. We call it "Q-methodology." This is a set of statistical, philosophy-of-science, and psychological principles which, we believe, is such as is demanded by the present scientific situation in the psychological and social sciences. Factor analysis is to be reformulated. With respect to the philosophy of science, we shall find Q-methodology in comport with logical analysis in all important methodological aspects, except such as have led to the excesses of *reductionism*. With respect to experimental method, Sir R. A. Fisher's great work on the design of experiments has contributed much here, as it has already done in other branches of science. These are the props upon which Q-methodology is con-

structed, and it is, of course, the purpose of this book to discuss them in some detail.

Our object has been to make it possible for studies to be undertaken on single cases.[1] We shall show how *any* person, in principle, can be made the subject of detailed factor and variance analysis. A great deal of reorientation and restructuring, however, is to be involved. Thus we shall make Fisher's methods, of small-sample theory and variance analysis, serve to represent our theories and explanations of facts in psychology. Factor analysis, on the other hand, will serve our experimental purposes. Matters are usually the other way round: Fisher's methods customarily concern experiments, and factor analysis, explanations. In experimental work the investigator chooses certain *independent* variables and reaches *dependent* ones as the outcome of his experiments. It has been maintained by all authorities that the correlation technique could not so distinguish between its variables: its concern was with the *interdependent* relations only—how A and B are correlated tells nothing, necessarily, about any causal connections between them. What the independent and dependent variables are, in such cases, cannot be specified. The correlational method, consequently, was not considered to be experimental in its procedures. We have found it possible to remove this restriction upon factor methods, and we are to describe in the following chapters a *dependent* use of factor analysis, so distinguished from the *interpendency* form long current in factor analysis. This does not mean, however, that we can point to causal connections unequivocally; rather, we experiment with causal possibilities in mind.

Many recent advances in the philosophy of science and logic of experimentation will also concern us in a major fashion. It is important, for example, to distinguish between general and singular propositions in matters of proof. Experimental work has reference, always, to singular situations—as when Newton watched a particular apple dropping from a tree outside his college window. Conclusions are then reached in relation to a theory. Traditional factor

1. By a "single case" we mean, for the moment, a single person under study or a single group of interacting persons. Later we shall see that what is involved is whether *individual differences* are postulated or whether singular propositions are being tested. The latter alone are our concern.

analysis, on the contrary, asserts general propositions—as when it assumes that everyone has every psychological attribute in some degree. The assumption is both unnecessary for scientific purposes and incapable of any proof. It is unnecessary because, without it, experiments can be conducted with "single cases," about which valid conclusions can be reached in relation to a theory. We do not mean by this that it is unnecessary to study *other* cases. Nor are we to outline a new principle of inductive inference which permits us to infer from "one to all." When the physicist theorizes about a particular metal, any piece of it will serve his experimental purposes. Likewise, for us, theories are matters of general import, the concern of a general psychology, and applicable to any person in principle. Nothing will be contributed by any use of "large numbers" of persons as such, for any purposes of statistical inference.

We are to be radical with respect to *theory*. Anything that could be called a psychological theory or hypothesis in traditional factor analysis had to wait upon the discovery of factors, the issue, as we have mentioned, of interdependency analysis. A virtue was made of necessity in this respect, and factorists have long taken pride in the pure objectivity of their procedures. But it can scarcely be denied that the consequence has been disastrous for theory. No general theory of a psychological kind is anywhere at issue in factor studies today, even though factor analysis was conceived originally in relation to one—Spearman, the inventor of factor methods, meant it to serve his theory of cognition (the *noegenetic* principles). General theories of this kind, however, lie about everywhere in psychology, lazy, like cats in the sunshine. Psychoanalysis is such a theory. Like most, it is not very systematic, deals with ultimate explanations, has no "ideal" type of operational constructs, and has no experimental methodology peculiar to it, to put its assertions to some proof in an acceptable scientific manner. But it is richly suggestive. We are to have some novel contributions to make with respect to such theories, as an outcome of Q-methodology. Modern logical analysis has been of inestimable value to us in this connection, providing us with principles upon which all else depends. The distinction between the generality of a theory (and the truth or falsity of a general proposition) and the empirical confirmation of any of its singular proposi-

tions is of the utmost methodological importance, and we shall deal with it later.

With respect to psychology as such, too, we are to be somewhat wayward. No prior place of importance is to be given, in these chapters, to the physiological or physical correlates of behavior. There is to be no concern with the brain, conditioning, the nervous system, or with cybernetic models of these physiological matters. Instead, we are to deal with concrete behavior as such, of the kind described by humanists, historians, playwrights, and novelists in a naturalistic manner and by some modern clinical, social, and industrial psychologists in a comparable, but scientific, fashion. The total person-in-action is our concern. One of our psychological principles is that what is *subjective*, such as thinking, and what is observable to *others*, such as playing golf, are in no way distinguishable for scientific purposes. Dreaming is as much behavior as is jumping a stile or dashing a hundred yards. All is a matter of interacting with this or that situation. Inner experience and behavior are thus alike. Both are matters for objective, operational, definition and study. We are to propose some first principles for this study.

In the past, although it has been little recognized, the concrete study of behavior has depended very largely, in its systematic respects (that is, without regard to physiological theories), upon assumptions about *individual differences* and upon the use of large numbers of "cases" as a basis for scientific operations and generalizations. The brilliant and penetrating analyses of famous novelists have been rejected from all scientific concern, because they deal, presumably, with particular cases or events. The use of the statistical techniques of *factor analysis* to study man's abilities, personality, and attitudes or the like has so far been acceptable, on the other hand, because greater generality appeared to be involved. We prefer the penetrations of the humanists and would like to bring them to heel, like the good hunters they have been, for our scientific purposes. This will involve us in *factor* methods, but always in relation to single cases, that is, to man in his concrete behavior, and not at all in relation to any *attributes* he may or may not have. No tests of the kind used for measuring intelligence and no *norms* based upon large samples of individual differences are anywhere necessary to our purposes. We require, instead, sound theories of behavior and

techniques for experimenting upon their consequences for any person we care to make the subject of scientific inquiry, or for any interacting group of such persons.

One or two matters of detail are worth mention at the outset. If the use of large number of cases has become dogma in psychology today, an exaggerated regard for *measurement* is no less a plague. It is widely believed that one must first measure a thing before it can be studied scientifically. *Scales* and *tests* of all kinds are therefore widely employed in psychology, although it is almost true to say that no one knows what he is measuring. We propose to throw away all such measurement. Yet we shall study man's attitudes, his thinking behavior, his personality, his social interaction, his *self*, his psychoanalytic mechanisms, and all else objective to others or subjective to himself; and we can do all this scientifically, without using any formal scales or measuring instruments of the kind with which psychology is familiar. This is achieved by Q-technique; and, of course, it is the purpose of these chapters to outline the conditions under which it is all possible.

It is widely believed that it is essential to work with large numbers of cases in psychology, so that valid generalizations may be reached. We are to work, instead, with a single person, at the call of a theory. Yet we shall reach valid, scientific conclusions. It will be objected, no doubt, that scientists have to generalize and that they can scarcely do so with any credibility from a "single case." From "one to all," however, is a very crude statement about induction. Thus X's eyes may be blue. We would not assert, therefore, that all eyes, of everyone else, will be blue. We might say, instead, that *some* other persons may have blue eyes like X. Given categories blue, gray, brown, green, etc., it is proper to ask questions of this kind: What proportions $p_1, p_2, p_3, \ldots$, of a population of persons have blue eyes, gray eyes, and so forth? The accurate definition of these proportions will certainly involve us in statistical issues and the use of large numbers of cases. But the fact of blueness as such is in no way dependent upon the facts of such proportions in any logical or inference-making sense.[2] The confounding of such different bases for one's inferences,

2. Certain relationships might be discovered between $p_1, p_2, p_3, \ldots$, such as Mendelian theory may suggest, but these need have no necessary concern with the operational definition of blue eyes as such.

or conditions under which inferences are drawn, has long been the bane of psychometry in particular and of psychology in general. The operational definition of proportions of the kind $p_1, p_2, p_3, \ldots$, is one thing; that for facts of the kind *blueness of eyes* is another. It is our essential objective to draw attention to many ways in which important facts about single cases can be reached, of a kind hitherto dealt with *theoretically* as so-called "explanations" or the like, which have a methodological status comparable in every way with the operational definition of such a fact as the blueness of a person's eyes. We require only a single case at a time, one, X, for our study—the operational facts about other persons will in no necessary way enter into conclusions we care to draw about X.

By the *operational* definition of a fact we mean precisely the kind of steps taken, in science, to assert that X's eyes are blue. Certain conditions of daylight and illumination are specified in the case, and lines on a spectrum or the like are defined as blue. In Q-methodology we make use of factor analysis, as well as other statistical methods, to define certain facts about X, such as a theory of personality or the like indicates may be worth attention. The statistics are confined to X, much as they would be to define the probable error with which a physicist may correctly assign X a certain grade of blueness of eye. Thus we might subject X to certain experiments, say in relation to his self-regard or to his attitudes about religious doctrine, and conclude that certain facts occur for him as factors $f_1, f_2, f_3, \ldots$. Our essential concern is with such facts. Whereas others have believed that little purpose could be served by being concerned with facts of this kind, confined as they seem to be to X, we propose, instead, that they can be highly credible if they are such as a sound theory recommends to our notice. This applies no less if we employ a few persons X, Y, Z, . . . , for the purpose of defining psychological *types*. Factors for a few persons, like those for a single one, may have considerable invariance attributable to them. Similarly, the factors discussed by psychometrists, such as Professor Thurstone, are defined in relation to one or two or very few mental tests and are thereupon thought of as invariant, with no further statistical, but some scientific, sanction.

Such, then, is the platform upon which we are to campaign. We have many principles to consider, the whole set constituting a

methodology. We claim no original authorship for many of the principles themselves, but the methodology as a whole, we hope, has some newness or is novel, if not radical in many respects. Some acquaintanceship with factor analysis, variance design, modern philosophy of science, and the broader issues of present-day psychology and the social sciences is, of course, indicated. We can make no apology for this seeming comprehensiveness: indeed, it is a nice compliment to American psychology that these foundations of its science can now be taken for granted. The concern is with far more than the simple operations called "Q-technique." Rather, it is with a comprehensive approach to the study of behavior, where man is at issue as a total thinking and behaving being. Our platform may be difficult to follow: but, then, so is any political platform difficult and sometimes unintelligible.

CHAPTER I

PROLEGOMENA TO Q

EARLY PAPERS

A BRIEF account of Q-technique is given in Sir G. H. Thomson's *The Factorial Analysis of Human Ability* (181).[1] In this he relates that the topic of correlation between persons was first given explicit attention independently and almost simultaneously by himself and the present author—by Thomson in an article appearing in July, 1935 (182), and by Stephenson in a letter to *Nature* (168), dated June 30, 1935. He reminded his readers, however, that the device had been used previously. But the wider methodological matters had not been grasped up to 1935; and, indeed, the fact that these have been in dispute ever since is testimony to the difficulties at issue, or to our obtuseness, or both. Thomson himself was very pessimistic about the technique's possibilities, for reasons that we shall see later, whereas we were highly optimistic.

Our first exemplification of the technique appeared in *Character and Personality* (Vol. IV [September, 1935]), in an article called "Correlating Persons instead of Tests" (162). In this we contrasted factor analysis of the time, with its search for "universal" factors (such as Spearman's *g*, *c*, and *w*, representing cognitive, affective, and volitional forms of behavior, respectively), with the new procedures we had in mind. About the former we had to say:

> Large populations of persons, however, are required in experiments that are made on these factors (*g*, *c*, *w*,), and as a consequence the quality of the testing suffers. The size of the population sets a limit on the tests that can be applied, and on the apparatus and controls that can be used. All the refinement and delicacy, the intricacy and subtlety of the laboratory, are lost just when it is most needed in the study of the most complex of all activity, human behavior. Experimental work is slowed down, almost to the point of being burdensome. A research student may spend two years isolating a single factor. All one's controls and hypotheses have to be sandwiched into the one experiment, to be performed on one and the same group of persons. One cannot perform an experi-

1. References are consolidated in one alphabetical listing at the end of the book.

ment today and use its results for another tomorrow. . . . In short, the present-day technique lacks the pliability that the energetic experimentalist wants at his command. It is a device for massive field work, and not for the clinic, the laboratory, or for rapid and subtle experimentation [162, p. 18].

The new technique, we suggested, made it possible to make factor studies on a single or a few individuals, thus bringing the methods of correlation and factor analysis into the laboratory and clinic. This was possible if *persons* were correlated instead of *tests:*

Whereas previously [we wrote] a large number of people were given a small number of tests, now we give a small number of people a large number of tests or test-items, or require a large number of responses from them [162, p. 19].

We proceeded to illustrate this possibility in terms of a simple experiment. A "sample" of 60 colored papers was taken, containing

TABLE 1

	Most Pleasing										Least Pleasing	
Score (X)	10	9	8	7	6	5	4	3	2	1	0	
Frequency	1	2	4	7	10	12	10	7	4	2	1	(n=60)

The most pleasing color is to be given score 10, the next *two* most pleasing score 9, and so on . . . , the least pleasing one gaining score 0.

brilliant poster colors, vivid reds, greens, blues, oranges, and many more delicate hues, but excluding whites, creams, or grays. They were handed to a subject like a pack of playing cards, previously thoroughly shuffled, and he was expected to look them all through and then to grade them, from those he liked *most* to those he liked *least*, in such a way as to conform to the prearranged frequency distribution shown in Table 1. Something of the kind had long been involved in the psychophysical methods. But our purpose was to correlate and factor-analyze such arrays, for different persons, or for the same one person under different conditions of experiment.

In a typical study twenty students each made such a set of appraisals, and the twenty represent the "small number" of persons to be correlated. The correlations were factored, which merely means that a basis was found for classifying the persons. If everyone's likes and dislikes are completely idiosyncratic, so that no two persons are alike in what they prefer, no significant correlations should appear and, of course, no factors. There will be correlation, however, if

different persons tend to like the same colors. In point of fact, that is what happened, and we could subsume the correlations by two factors or classes, twelve students belonging to the one, and eight to the other. The one set placed vivid, lively colors high in their order of preference, and less highly saturated *low;* the other class appeared to prefer rather stereotyped colors, irrespective of saturation.

This, we said, was "just a beginning." For it was clear that this simple device could be used as a means for studying much indeed in the realm of psychology that could not otherwise have been reduced to operations. "Introspective psychology in general," we averred, and "our preferences, sentiments, motives and ideals," were now open for the technique to explore (162, p. 19). Work on *individuals* was referred to, who "could be asked to select those [art objects] that they believe to be the most tasteful, instead of merely the most pleasing, to see whether knowledge, pleasure, and taste are associated" (p. 23). We clearly distinguished between such a possibility and the testing of a selected *group* or groups (p. 23), and as clearly distinguished between such applications and the use of the technique to check the theories of "type" psychologists (p. 24). But in no way was it suggested that the methodology consisted of discovering *factors* or the like. Instead, we clearly envisaged (p. 23) the orderly development of *experimental* studies, such that many indeed would be needed even about a relatively simple matter, such as *taste* for colors.

Several studies of this illustrative kind were being pursued in 1936 (163, 172), to which Thomson drew attention. It was at this point, however, that questions arose as to whether the correlation of persons was essentially *new*, in a psychometric sense, different, that is, from the more familiar technique of correlating *tests*. From the outset what we had in mind was indeed new; but Sir Cyril Burt (41) thought otherwise. Thomson was cautious, although his arguments supported our standpoint rather than Burt's.

We should try, perhaps, to recapture the preoccupations of the time and to see why the question of Q-technique's independence of R-technique (as the correlational analysis of *tests* and individual differences was soon called, to distinguish it from Q-technique and the analysis of single cases) should have occasioned such differences of judgment and opinion. We provided in 1936 many examples of

tables of correlations containing large negative values—about which no one would be surprised nowadays—but which was discussed by Thomson in the following way:

> If ever correlations between persons come to be analyzed as minutely and painstakingly as correlations between tests, it would seem that the free admission of negative loadings would be necessary [181].

In concluding his brief account of correlating *persons*, Thomson added the following:

> It remains to be seen whether factors which will prove acceptable psychologically will be isolated in persons [181].

These statements, we believe, betray the methodology of fifteen years ago, against which we began to object. It stems, we suggest, from Karl Pearson's *Grammar of Science*. In this the whole wide universe, the geography of the earth, its flora and fauna, and every human action, were grasped by Pearson as a vast matrix, cemented together inexorably by correlation. From the equator to the poles, trees become stunted. Skirts lengthen with economic depressions. The stars seem held in place by intercorrelations. Everywhere correlation occurs. Similarly with respect to mental tests and individual differences. All of which is no doubt true enough. But it is another matter to suppose that this *correlation* has only to be studied in its own right in order to reach great scientific truths. This, it seems to us, was the underlying belief of the traditional factorist. We were criticized only recently for violating it: "Stephenson is prepared to forego the significance of correlation or factors, for the advantage of taking a few extreme cases and studying the constellations in which they emerge from factor analysis." The word "significance" really means what it says here—correlation *as such* and its factors are at issue, regarded as given, somehow, in the vastness of all possible matrices of individual differences. It has merely to be grasped by statistical inference. One could not experiment upon it but only describe it as it really exists. It was indeed precisely our purpose to forego such conceptions; and, incidentally, we also study *any* person in principle. Thus it seemed obvious to us that the correlation of *tests* (R) had its roots in protopostulations of this kind and in a corresponding belief in *abilities*, *capacities*, *potentialities*, and similar general propositions, and that R-technique could have no meaning except in terms of its postulates about *individual differences*. These

are assumptions upon which all else depends: with others of the kind, many of a protopostulatory nature (so deeply ingrained in the factorist's thinking that it takes a very special effort on his part for him to become aware of them), they constitute a complex *system* or, as we now call it, a *methodology*. We call it "R-methodology," and much of it will receive attention later.

It was our purpose to provide Q with its own set of definitions and postulates, for reasons that we could grasp at once in terms of much that had been discussed in psychology for nearly a hundred years and which had never had a satisfactory answer up to then. We considered it important to rid at least one factor technique from, on the one hand, any postulatory dependency upon individual differences and, on the other, from protopostulations of a seriously limiting kind, upon which R was based. We had therefore to construct a methodology for the single case: we call it "Q-methodology."

The reasons for this are not far to seek, in terms of the background that a Spearman student had to have in the latter part of the 1930's. For we were oriented toward the history of psychology, and we knew our Ward (197) and the cross-currents that were critical of the "elementalism" that conceived of man as a mass of "characteristics," such as his height, temperament, intelligence, and the like, which could be studied in terms of individual differences. Spearman (149, 150) himself could not grasp the force of these criticisms. He regarded *gestalt* psychology, for example, as a sort of "public enemy No. 1." Besides, he was fascinated by the elegancies of R-technique, which offered to deal sensibly with man's "characteristics" in a way hitherto quite impossible of achievement. But at least one of his students strayed: Q-technique, we were sure, could represent the newer, but really old, viewpoint of a Dilthey or a Ward. It would do this if it could deal with "wholes," with "descriptions," with the "concrete person."

Thus, from the outset, we regarded R- and Q-techniques as far more than the mere statistical matters which others, it seemed to us, believed them to be. This was made clear, we thought, in a paper which appeared in *Psychometrika* in 1936 (171), called "The Foundations of Psychometry: Four Factor Systems." The introduction to this paper, indeed, had precisely the following to say:

> In the present paper, after defining the [four] systems and supplying an example of each, I proceed to examine the modes that each can subserve. . . . Two of the four are statistical statements of a relativistic standpoint, and may be of help to *gestalt* psychologists, whilst the other two are parallel foundations for "elemental," process, or ability psychology [pp. 195–96].

The years 1937–38 and thereabouts were spent, in part, in an exchange of views between Sir C. Burt and the present author about the various pros and cons of correlating persons. The outcome was a paper by Burt (39), a setting for his *The Factors of the Mind* (41), in which there are many and detailed references to our respective viewpoints. We also wrote jointly, in a paper which appeared in *Psychometrika* (44). Several other papers followed (156, 157, 161, 163, 164, 167, 172).

It would take us into unnecessary detail, at this juncture, to enter into even a small number of the issues upon which Burt and the present writer differed. They are on record in the above papers, and the purpose of this book is to elaborate upon our own viewpoint, as Burt has already done about his. Burt's essential preoccupation was with the possibility of relationships between factors for *persons* and for *tests, for one and the same matrix of data*. Thus it seemed to him that the factors for *tests* should bear some relation to those obtained in terms of *persons*, for the same data. He proved, indeed, that factors are identical for tests and persons (43), under certain conditions. The reciprocity at issue is readily understood from the following simple demonstration:

In factor theorems a person's score x in a *test* may be divided into any number of parts, and, in the simplest case, it might be represented as

$$x = m \times k\ .$$

Here m is the factor loading of the test and k the score gained by the person in that factor. The proportioning, of course, is not arbitrary. Theoretically, m has reference to the "goodness" or "soundness" of the test for measuring the hypothetical factor in question—empirically, for example, we would expect a test of *reasoning* to be more highly loaded for a factor of intelligence than a test of *canceling letters*, other things being equal. Similarly, k has reference to the supposition that there are individual differences for the factor—some persons are brighter than others. The expression, in short, is as

fully representative as that for a falling body, viz., $S = \frac{1}{2}gt^2$, where g is by no means arbitrary but relates to gravitational theory.

The situation for three tests and four persons, then, would be as shown in Table 2. In R-technique the data in each column are standardized, i.e., R-values are in standard terms,[2] and it is easy to prove, as Spearman did, that the correlations between the *tests*, r_{ab}, r_{ac}, and r_{bc}, are as follows:

$$r_{ab} = m_a \times m_b \,, \qquad r_{ac} = m_a \times m_c \,, \qquad r_{bc} = m_b \times m_c \,.$$

But if we look along the *rows*, the k-values are now constants, and the m's could constitute variate arrays. What were factor loadings in the tests, (m), in *columns*, become factor scores for the tests in *rows;* and what were factor scores for persons in *columns*, (k), may be re-

TABLE 2

Persons	a	b	c
1	m_ak_1	m_bk_1	m_ck_1
2	m_ak_2	m_bk_2	m_ck_2
3	m_ak_3	m_bk_3	m_ck_3
4	m_ak_4	m_bk_4	m_ck_4
Means	0	0	0

garded as factor loadings for persons in *rows*. This, essentially, is what Burt was pointing to as the reciprocity of factors for persons and tests.

Our own position was that, although this reciprocity applies to any one matrix of data, the more fundamental concern is with two different matrices—one of which is based upon postulates of one kind and the other upon postulates of quite a different order. The one was our *system* (*1*),[3] of the early paper (171), or R-methodology,

2. I.e., the *means* are 0 and *standard deviations* 1.0 for each test.

3. These *systems* are referred to in more detail in chap. iii, pp. 51 ff. In system (*1*) the concern is with samples of *persons* to which tests have been applied: the analysis begins with the *individual differences* so observed. The system thus subserves differential psychology fundamentally, and all the factor work of Spearman, Thurstone, Hotelling, Holzinger, Burt, Cattell, and others falls within this rubric. In system (*2*), samples of *statements* are constructed, which are used, basically, for statistical descriptions for a single person: the concern is with *intra-individual* "significance" of these statements. In (*1*) the *tests* are correlated and factored; in (*2*) the *descriptions* are so dealt with. Systems (*3*) and (*4*) are merely alternative ways of looking at data for systems (*1*) and

as we now call it; the other was our *system* (*2*), or Q-methodology. These are not, and never can be, reciprocals of one another. But we defined what was reciprocal to system (*1*) and also to (*2*), namely, our systems (*3*) and (*4*), respectively. Ever since, every factorist seems to have been bent upon disregarding these differences. Thus we find one of our most recent critics writing as follows:

> Another field of interest for psychologists, over which battles have been fought and will no doubt be fought again, is the field of R- and Q-techniques. If R-technique is concerned with analyzing the correlations or variances between columns in a matrix of scores, then Q-technique is concerned with analyzing the correlations of variances between rows. That is to say, where R-technique is concerned with the relationships between tests, Q-technique is concerned with those between the persons who take the tests [12, p. 80].

One can merely say about this that it is in *flagrante delicto*. There never was a single matrix of scores to which *both* R and Q apply. Naturally, with such an Aunt Sally, our critic "can see no reason for the controversy" that has developed "round this complementary procedure."

Thus, in R a number of *tests* are applied to a sample of *persons*. The tests are scored objectively, and the concern, fundamentally, is with *individual differences*. But if we now invite any person to look at all the tests, to deal with them as he would with a pack of cards, and to order them with respect to (A) which he believes himself to be most *expert* at, or (B), which he would *like* most to do, or (C), which, in his opinion, is the *fairest*, or (D), which will give most information about his *ability*, and so forth—in any such case Q-technique would be at issue, and the concern, fundamentally, would be with the *single case*. For we could take *any* one person and make him the subject of experiments ad lib. The *test material* (but clearly not any results they provide vis-à-vis individual differences for a sample of persons) would be our sample, and these different conditions of instruction, A, B, C, D, . . . , would be the point of origin of our *variates*. We would correlate these variates and factor the correlations. This would be an application of Q-technique. As we see, even if the material is the same, as the tests are in this example, it

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(*2*), respectively, and they are thus not independent of (*1*) or (*2*), respectively. System (*3*) comes within the purview of *profile analysis* (Cronbach, 55); it also leads to Cattell's P-technique (48).

would be differently employed; in R the tests are meant to measure *abilities* by way of individual differences; in Q they would be used to experiment upon certain attitudes of mind of *any* person we cared to make the subject of inquiry. In Q-methodology, of course, we devise all sorts of samples for our experimental purposes, and only rarely would mental tests be used like a pack of cards as in the above example.

We therefore find the following statement by Burt (12) fairly acceptable about these matters:

> (i) Where a project is designed in terms of tests which are applied to a sample of subjects and where the correlations between tests are subjected to analysis, one may talk of R-technique.
>
> (ii) If with the same data, one runs correlations between persons instead of between tests, then one has P-technique.
>
> (iii) The innovation, which Stephenson claims as Q-technique, is to design an experiment in terms of people . . . then to assess qualities of performance with respect to each person in turn, and then to make correlations between people.

The P-technique, to which reference is made, is merely our *system* (*3*) again; that is, data put together for R-purposes can also be analyzed in the reciprocal manner referred to earlier (Burt's reciprocity principle [41]). We would add to (iii) that the experiments are designed, in principle, about a single person—*any*, in principle.

Our objective, then, is to develop the thesis that methodologies are at issue and not merely a few statistical theorems, such as Burt and our critics have supposed. The methodologies, indeed, involve psychological, logical, experimental, methodological, and philosophy-of-science principles, besides many statistical ones. It is such matters that we have to consider in order to resolve R and Q. Psychology has recently become conscious of its methodologies. One need only refer, in this connection, to the *hypothetico-deductive* principles of the late Professor C. L. Hull (83). But psychometry, much earlier, had a methodological status (R) upon which has been developed all standardized mental testing, the measurement of abilities, skills, and temperamental qualities. James Ward (197) gave his penetrating attention to its assumptions, as has Burt (41) to a lesser extent, more recently. Q-methodology has pretensions to this same scale of things. It is essentially a new set of principles, collected together to suit the scientific situation in psychology today: we call

it a *postulatory-dependency* methodology rather than *hypothetico-deductive*, even though it has much resemblance to the latter.[4] The designation Q, however, will serve our purposes as long as Q-methodology and Q-technique are distinguished, since the latter is only one of many operational procedures that may subserve the wider methodology.

SOME EXAMPLES OF Q-METHODOLOGY

A few brief examples of Q-technique, and of the methodology it serves, will not be out of place. We begin with an example of a simple kind which has reference to the Szondi Test (59). This, as is well known, consists of 48 photographs, each on a card showing the face of a former mental patient, 8 each for former sadists, hysterics, catatonic schizophrenes, paranoids, depressives, and manic patients. The test itself is a projective instrument and not the kind that

TABLE 3

	Most Liked								Least Liked	
Score	8	7	6	5	4	3	2	1	0	
Frequency	2	4	5	8	10	8	5	4	2	($n=48$)

R-technique was ordinarily concerned to study. If we regard the 48 cards as a sample for Q-technique purposes, they can be thoroughly shuffled (as for a pack of cards in bridge) and then quantified in many different ways. Thus we might ask a choirboy, who has, of course, no idea what the cards really represent, to look at them all, and then to *score* them for (A) which he *likes best*. To help him along we would provide a frequency distribution for him to work to (Table 3). Thus he gives 8 marks to the *two* photos he likes *most*, and 0 to the *two* he likes *least*, and so on, with most of the cards gaining 3, 4, or 5 marks in the center of the distribution. The act of so scoring a sample of cards is called a "Q-sort," for short.

But we might also invite our choirboy, next day perhaps, to score

4. Our procedures are in some respects more elaborate than those followed in the hypothetico-deductive methodology as ordinarily employed, e.g., by Hull. It is well known that many modern philosophers support the hypothetico-deductive principles, for the most part (Wisdom, 200); but the methodology has some serious limitations, to which Kaufmann (92), among others, has attended. For these reasons, therefore, we have not used the term "hypothetico-deductive" for Q-methodology.

the same cards for (B) which he believes to be the most *godfearing*. Or (C) which is the most *handsome*. Or (D) which is the *healthiest*. Or (E) which is the *oldest*, and so forth. In each case the same frequency distribution might be used, the cards themselves, of course, usually gaining quite different *scores* under these varying conditions of instruction. The arrays of such scores, for A, B, C, . . . , can be correlated ($n = 48$) and factored, all with respect to the same choirboy. This, in miniature, is the kind of data dealt with in Q-technique, and it seems straightforward enough. But there should be a theoretical reason for the study, which will determine what the particular conditions of instruction will be and why this particular choirboy is the subject of experiment. If he happens to be suspect for schizoid condition, for example, we would be wanting to see, not what *cards* he selects as such in relation to norms, but what relations, if any, exist between the various Q-sorts he makes under the various conditions of instruction. That is, the interest would be to probe in a more detailed and individual manner into the particular case. Moreover, we would do the probing in terms of Szondi's theoretical framework all the time. Indeed, along such lines it is possible to put Szondi's theory to test, by way of detailed studies of single cases, using the factor analysis (and, as we shall see, *variance design* as well) as a statistic for testing propositions about the case in question. Such in a very brief outline is Q-technique.

Or, to take another example, following one of our early studies, we may wish to examine what happens when a student X attempts to solve a series of jigsaw puzzles which are complicated enough to permit of observations being made (i) by outside observers, of the manipulatory behavior displayed by X, and (ii) by X himself, of his own behavior. In the latter case, or indeed in either case, one could also wonder how the student *felt* about the situation. For studying such matters we can compose, say, 60 verbal statements, descriptive of manipulative behavior, of the following kind: (1) picks up pieces randomly, (2) makes the same mistaken moves over and over again, (3) appears to hold clues in mind, and so forth. Again if each such statement is written on a card, the cards can be shuffled; and observers, as well as the student himself, can describe the latter's performance in terms of the statements by marking them for (*a*) their *incidence*, (*b*) their *significance*, (*c*) the *frequency* of their occur-

rence in the performance or the like conditions of instruction, using a forced frequency distribution of scores of the kind just referred to for the Szondi cards. Again the arrays of scores for the $n = 60$ sample can be correlated and factored, and again one could undertake such studies for theoretical reasons, such as to examine questions about insightful performance.

So we could continue with examples in many different regions of study, with respect to aesthetics, attitudes, thinking behavior, self-reflections, and every conceivable form of human behavior, individual or group. In every case the interest would be focused upon the *theoretical* matters, and Q-technique is just a *modus operandi*, a way to test the theoretical issues. At its face value, indeed, the technique is eminently simple, and one might well wonder what all the fuss and bother are about. First a "universe" of statements or the like is defined (such as many possible Szondi cards, or performance statements), that is, as postulatory matters. A sample of these is then quantified by operations upon them, relative to one another, constituting a Q-sort. Then Q-sorts for one person for different conditions of instruction, or for different persons for the same or different conditions of instruction, are correlated and factored. However, these simple matters entail many unsuspected methodological postulates and protopostulates, and it will take all the chapters of the book to elaborate upon them.

Let us look at another example in a little more detail. Suppose that a person, X, who might be a plumber, a priest, a schoolgirl, or the like, writes an account of his or her personality. It would consist of statements of the following kind: (*a*) I am generous to a fault, (*b*) My failings are many, (*c*) I sometimes feel like running away from everything, etc. If X is clever enough, we might get several descriptions from him, each for a different condition of instruction, e.g.:

C_1 Write about yourself as you think you are usually.
C_2 What do others think you are like?
C_3 Write about your inner aspirations, etc.

Q-technique deals with such *statements*, $a, b, c, \ldots$, and *conditions* of *instruction* $C_1, C_2, C_3, \ldots$, in a formal manner.

It might be supposed, or be the reason for our study, that X will "give himself away" in making these different self-descriptions,

perhaps *projecting* without knowing it or otherwise revealing himself in unsuspected ways. It is the object of Q-methodology to examine such possibilities. We distinguish between the *theoretical* and the *operational* procedures at issue. The former will be dealt with by way of factorial designs such as R. A. Fisher employs. The latter will involve dependency factor analysis. Facts of an explanatory kind will be dealt with by way of the Fisherian designs, whereas propositions will be tested by way of factor analysis. These are important methodological matters with which subsequent chapters will deal in some detail. A simple operational procedure for the case X above could be as follows: The literary descriptions are first replaced by purely statistical ones. Thus, for C_1, and say 100 statements, *a*, *b*, *c*, . . . , the subject X scores the latter in relation to one another, giving each of the 100 a score to represent its importance or

TABLE 4

Score	10	9	8	7	6	5	4	3	2	1	0	
Frequency	2	4	6	10	16	24	16	10	6	4	2	($n = 100$)

"significance" in the total description. Next X is expected to array his scores so as to fit a "forced" frequency distribution, for example, Table 4. Thus for C_1, X may find himself giving 8 marks to statement *a*, 5 to *b*, and 2 to *c*. For C_2 the scores for *a*, *b*, and *c* may be 4, 3, and 10, respectively. Clearly, under different conditions C_1, C_2, C_3, . . . , the *statements* may receive very different scores. The arrays of such scores for different conditions are correlated and factored. There could be, say, 20 different self-descriptions by X, each for a different condition of instruction C_1, C_2, C_3, . . . , C_{20}, and each made with the same 100 statements *a*, *b*, *c*, ... The 20 arrays so achieved constitute 20 *variates* for correlational purposes, providing a 20×20 table, requiring us to calculate 190 correlation coefficients. The sample size is $n = 100$ (the 100 statements). In effect, then, the $20 \times 100 = 2{,}000$ scores are subsumed as 190 coefficients. Factor analysis results in their further subsumption as, perhaps, 3 *factors*, f_1, f_2, f_3. Since each of the 20 variates may have a factor loading, the 190 coefficients would be subsumed by $3 \times 20 = 60$ *factor coefficients*. The 2,000 scores, so to speak, are reduced to 60 only. But the

factors make it possible to provide three "explanations" of all these data. It is these, and their interrelations, that give factor analysis its chief interest. We might explain factors f_1, f_2, and f_3 along psychoanalytic lines: f_1 may be regarded as *rationalization* behavior on the part of X, f_2 a habit of *projection*, and f_3 an *idealization*. With the psychoanalytic theory in mind, it may be quite compelling and apparent that the factors may be so interpreted; and we may draw the inference that each is indicative of a habitual mode of behavior of more or less stability. These inferences are made in a theoretical context of knowledge about psychoanalysis and human behavior.

We have described the procedure, so far, as though purely empirical matters are at issue—it seems that factors are first found and then explained. Theory, however, is involved all the while, at three places. First, it indicates what the sample of statements will be initially: it defines the "populations" of statements for us in Q-methodology. Second, from the theory certain propositions are ordinarily derived, and, in turn, the variates are chosen which will put these propositions to test. Third, the theory is used to guide us in the factor solution, telling us what sort of facts to look for. We can rotate factors until the facts are found, if they exist according to expectancy. The inductive principle in factor analysis is thus reduced to the modest proportion of a conclusion about a factor, based on prior theoretical expectancies. It is no longer used as an absolutist principle which, giving rise to factors in simple structure, leaves it to the factorist to offer explanations, if he can, a posteriori, or by hindsight.

We can return to the matter of generalization. It would be foolish to assert that the factors f_1, f_2, and f_3, found for X, will be found for *all*, or even *some*, other persons. Each person's factors, indeed, could be quite unique, operationally, to him. What degree of *invariance*, i.e., generality, to apply to f_1, f_2, and f_3 for X *himself* is also another matter. But here again, at this microscopic level, large-sample doctrine for X's operations is not crucial or essential to the inference that the factors are or are not likely to be invariant. Knowledge of a theoretical kind about X can help us assert, for example, that "f_1 is likely to be a stable rationalization employed by X," and that "f_2 is clearly a projection in relation to known facts about X," or that "f_3 is a habitual mode of self-regard by X." Such inferences are made in

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relation to the *total scientific situation*, an important concept to which attention will be given later. This does not mean that we leave such factors "up in the air." We can proceed to show that the factors can be repeated for X, at any moment we like. Thus we can call upon him to give another self-description, C_{21}, which may be different from C_1 to C_{20} in instruction, but which, we can predict, will be loaded with factor f_1. Similarly for all other factors. Lawfulness is thus at issue for X.

But *facts* of this kind are likely to be found for other persons as well if the theory we use and our operations are on the right lines. It is this that is "general" with respect to different persons. It is in no way supposed that the same facts will be found for every case so tested—fetishism, for example, may be highly particular to certain persons. But that some such facts will emerge along these lines is an obvious gamble on the scientist's part, the odds against him being almost negligible in the example we are using for the sake of illustration.

In principle, the kind of facts opened to our regard in the above manner are, of course, open to extended or intensive further exploration and experimental study. What has been achieved, up to this point, is really this—that a person X can be made to "give away" many habitual modes of behavior in Q-sorting operations, in such a way as to have them operationally defined, i.e., so that they can be subjected to experimental study and all the procedures of sound scientific method. Whether the theory, such as psychoanalysis, is acceptable or not is quite another matter. Better theories may be at hand. But no theory can be proved or disproved in any simple manner, at least no theory of the kind we have in mind in connection with the study of behavior. Rather, for the present, we should judge a theory by the interest and pertinency of the facts we can discern and "explain" in its terms. We have described the methodology as it applies to so-called "subjective" data, of a person's self-descriptions. But it covers behavior of all kinds, whether of the self, or of observations made upon others, as later discussion will show.

A BASIC PSYCHOLOGICAL PRINCIPLE

What is subjective, however, and what objective are worth a few moments' consideration. Scientific method is *objective*, we say, in so

far as testable operations are involved and reliable events. But the word "objective" is also used to mean "as observed by others," the contrast being with one's own "inner experience," which can be observed (it seems) only by the experiencing person himself. The word "subjective" has the same bifurcation of meaning. It means either inner experience or the opposite of scientifically objective. The split in meanings occurs, no doubt, because of the apparently unreliable and unstable nature of one's inner experience—subjectivity is fickle, to the highest degree of irresponsibility, so it seems. What is subjective, moreover, is never open to public regard. So thus it fails on two counts to be worthy of scientific regard—it is unreliable and is never observable by others. It has thus become a cliché of behaviorism and of much American psychology to regard subjectivity as *non*scientific per se, or *un*scientific *in esse*.

The bifurcation into *mind* and *body* parallels, of course, that just considered. Psychology, in its long history, has attended to the mind as its essential concern. The writer was taught his first psychology by Spearman (149), the last, perhaps, of the classical systematic psychologists; and the principles of McDougall (113), Ward (197), and Stout (178) were his daily fare twenty years ago. These psychologists believed in a substantive mind and consciousness. Nor had McDougall, the originator of the term "behavior" in its psychological sense, a prescience of anything other than this same belief. Logic, however, leads us to regard behavior as neither mind nor body nor physiology: it is simply behavior, whether subjective to a person or objective to others.

There is more in this, however, than meets the eye. We wish to study behavior as such. Behaviorism, also, claimed as much for itself. But by 1940 there were, on the American scene, many different forms of behaviorism—molecular, molar, operational, purposive, operant, and the like—none of which included subjectivity in its system. Gradually, indeed, the word "behaviorism" has come to mean the study of man's objective behavior, that is, excluding his subjective activities. It has indeed increasingly become linked to the study of physiological aspects of the problems of behavior. Much, therefore, is confusing: scientific psychology today seems to have set a course for itself which must end in its reduction to physiological processes, or to cybernetic models of the brain and the like. Yet

these matters, although arbiters of behavior perhaps (208), are no more its conditions than the beauty of a garden is the bacteria and chemical substances of its soil. We can see through the garden to its soil, and through behavior to the brain. But perhaps the garden as such and behavior as such are worth some attention from the inquiring scientist.

The essential clarification about the study of behavior comes from an American source, from the writings of J. R. Kantor (88, 89, 90) and A. F. Bentley (21). Their first glimpses have been grasped more clearly, recently, by some logical analysts. Kantor had little more than a first broad postulate (*interactionism*)[5] and a few supporting definitions (such as *behaviorial segment*,[6] *interactional setting*,[7] and *contact media*[8]); nevertheless, these bare beginnings were essential and provide the starting point from which any science of behavior must develop. It is true that Lewin's topological and vector notions (104, 105) have gone much further along Kantor's lines since the latter's earliest formulations of interactionism. But Lewin soon "encapsulated" much of his psychology (Brunswik, 35); the field of subjectivity of inner experience, for example, was altogether detached by Lewin from the field theory with which he sought to study groups of persons in interaction.

It was A. F. Bentley, therefore, who made possible the more adequate principles, which he did by taking up matters at a point at which they had been left by Kantor. In his *Behavior, Knowledge, Fact* (21), Bentley put at least one issue in behaviorism upon a

5. "Transaction" and "interaction" have reference to the way in which an organism and its surroundings mutually affect each other. Thus a person X may be composing designs with pieces of colored paper. He contributes something to the design, but the pieces of paper do so as well. If X is unduly preoccupied with his own troubles, he may *project* something of these upon the design, without knowing it. If he is of rigid disposition, the design may mirror it. If he is truly artistic, on the other hand, he may *sublimate*, that is, use the colored papers in such a way as to involve *their* intrinsic qualities as well as his own. The term "transaction" is sometimes reserved for the latter form of interaction.

6. Running a race or writing a letter is an example. Such segments have poorly defined beginnings and ends. All have roots in past behavior, i.e., in one's history.

7. Running 100 yards is one thing in a race, another when a lion is behind, and still another in a dream. Each has its own interactional setting.

8. This refers to sound waves, light, and the like, which are involved in stimulating an organism's sense organs.

systematic footing, and it is from this that a stand has been taken in the pages which follow. This concerns the various regions in which operations can be conducted, that is, in which propositions can be tested according to the rules of scientific procedure by proving, disproving, or falsifying them (92). Lewin imposed two kinds of regions upon his formulation, as we have seen, one of which was similar to the common-sense world of real objects, whereas the other was the encapsulation of each person's own inner and private experience. Bentley argued forcibly against "isolationalities" of this kind. Science, it seemed clear, has to find one region for its operations: if behaviorism had to become widely acceptable, it, too, must fit into this one region or find one region for all its operations.

From our own standpoint we argued, in the same way, that, as Q-technique could be applied to subjective as well as objective behaviors, there could be no valid basis for their separation. At least we could see none. We had found a region common to all such behaviors, namely, one concerned with the empirical testing of propositions about behavior, whether "inner" or "outer." It was highly gratifying, therefore, to find that the thinking of many logical analysts was pointing in the same direction—including philosophers of the stature of Wittgenstein and Schlick, but excluding, for example, Bertrand Russell. The matter, indeed, has been made more explicit recently in Ryle's book *The Concept of Mind* (134). The conclusion is that mind and consciousness do not exist in any psychic or "experiential" sense. There is not one realm of a ghostlike mind and another of body. All that the psychologist can concern himself with is *behavior* (63). This is not to say, however, that man's *subjective* behavior does not exist. Certainly he thinks, feels, imagines, muses, dreams, and all else. All such is behavior, every bit as certainly as is his purposeful walking from one place to another or his toying with a ball. In so far as this subjective behavior can be made amenable to reliable operations, scientific method is at issue and, in that sense, objective procedures. This is precisely our position in Q-methodology. Along Q-lines all subjective behavior, hitherto regarded as *in esse* arbitrary and unscientific, is capable of study with full scientific sanction, satisfying every rule and procedure of scientific method. The counting of pulse rate by a physiologist differs in no methodological respect from the counting of X's *factors*, in the example to

which reference was made earlier. Yet in the latter case we seek to delve into X's subjective behavior, in all its apparent complexity. Fluctuations in a person's skin resistance (the so-called "psychogalvanic reflex") are not observable to the naked eye but require electrical equipment for their discernment: our *factors*, in Q-technique, which merely indicate different modes or habits of subjective behavior, are every bit as instrumental and as objective in principle.

Thus Bentley's thesis dropped pleasingly into our scheme of things, like the last piece of an unfinished puzzle. Psychology, for us, can know no boundaries, in principle, between what is subjective behavior and what is outwardly observable behavior. We shall use the term "behaviorism" in the future, therefore, with this conclusion in mind. The issues concerning "experience" and behavior are considered in more detail in chapter v.

Important consequences follow. It will become clear, as we proceed, that so-called "self-psychology" comes within our rubric of behavioristic science. Much, therefore, that has been discarded by the older behaviorists as subjective and incapable of scientific study returns to the scientific fold for orderly empirical exploration. Meanwhile, psychology seems to have leapt forward amazingly along physical-physiological lines. The striking resemblances (208) between some behavior of animals and electronic models of the kind dealt with in cybernetics are highly attractive to all of us who believe that the same basic principles must be common to all the sciences, whether physics, physiology, or psychology. It may seem, therefore, that the methodology to which we are directed in this book has come too late in the day and that the fundamental work of psychologists must lie in the pursuit of electronic models of the brain. We doubt such a hasty conclusion. On the contrary, it is perhaps as important for us to isolate the habits of thought and behavior of man: the electronic models may follow if they can. It seems to us that much indeed remains to be discovered about behavior, more especially of the subjective kind, which has never had operations so open to it as now.

SOME REASONS FOR A NEW METHODOLOGY

R-methodology, or psychometry, as it has become, has a certain fascination for those who believe that objective measurement is al-

most the be-all and end-all of science and who search for universal parameters of, we suppose, the human mind. The methodology is rooted in a standpoint to which a certain aura of respectability is attached, namely, differential psychology, or the study of individual differences. This was acceptable to some psychologists because of its "objectivity," its basic "soundness," its systematic and apparent objective nature; and, indeed, it now seems clear that some of its advocates can think in no other terms and regard the assumptions of individual differences as obviously true, inescapable, and completely basic to all else. Q-methodology, however, had no such psychological fashion and fixation to support it or to give it contemporary scientific meaning. Or so it seemed ten or fifteen years ago. Such fashions as we could appeal to in early papers (162, 163, 171, 172) were already very largely discredited and outmoded, as appeared to be the case for the *type* psychologies, for example, of Jung (87) and Spranger (152). Even the tests that Q-technique could attend to and support, such as the Rorschach (132), were widely regarded as esoteric, unscientific, and scarcely worth a moment's serious attention by "objective" psychometrists. It seemed clear, therefore, that Q-technique required a distinct psychological orientation in order to give it wider credibility and acceptance. It was clearly useless to revive older viewpoints, even for illustrative purposes. The opportunity had to be taken, instead, to present the technique in terms of current psychological notions and principles, which, at the same time, would serve to increase one's understanding of and insight into Q-technique. It is easy to appreciate, therefore, why the methodological considerations of Bentley, Kantor, Wittgenstein, Schlick, and the logical analysts, with their support of a more comprehensive behaviorism than ever Watson conceived, fell like manna upon the amorphous desert in which we moved. These suggested what the psychological principles should be. But it is no less true that Q-technique has been effective in bringing many issues into clearer focus and in making obvious what had hitherto been confused or the importance of which had been overlooked. We begin, then, with an innocent enough technique, but we end by formulating afresh many methodological and psychological matters of great interest.

There are other reasons, too, for involving ourselves in methodological matters. There were two or three chapters in James

Ward's *Psychological Principles* (197) which are fascinating. They denied, in effect, any scientific validity to R-technique and called, instead, for quite a different schema for studying the concrete human being instead of, as in R-technique, his concatenation or assemblage of attributes or characteristics. Q-technique, we guessed at once, was the necessary and sufficient schema that Ward looked for but could not find. Again, those who have read Sir C. Burt's *The Factors of the Mind* (41) will remember that large sections of it concern logical and methodological issues. These in part seem to be completely up-to-date, with quantum theory and relativity thrown in for our delectation. But more of the argument in this monumental work leans heavily upon nineteenth-century rather than twentieth-century thinking. Frequently, the methodological matters to which Burt appeals are unacceptable to present-day thought about them. This is the case, for example, with respect to Burt's treatment of such matters as *inductive inference*. Someone, we felt, must bring these issues in factor analysis down to earth.

Furthermore, we were never one of those who believed that mathematics or statistics could give birth, per se and parthenogenetically, to matters of necessary psychological interest and importance. Factor analysis is concerned with two very different forms of multivariate analysis, one called *interdependency* and the other *dependency* analysis, to which reference has already been made. The former might be called the "blind inductive" approach, and the latter the "open-eyed deductive." Interdependency analysis has to do with the discovery of relationships among data no part of which is regarded as of prior importance over any other—the analysis produces factors, *in puris naturalibus*, which may, or may not, have any concrete reality. Theoretical or abstract matters are at issue, which might have as little tangibility as Kantian *noumena*. The dependent forms of factor analysis proceed very differently, the concern being with effects which are specified beforehand and which one wishes merely to put to an empirical test. The assertion is made, or implied, that an independent X causes changes in a dependent Y. R-technique, for the main part, was conceived as a matter of interdependency analysis; Q-technique has no such pretensions and restricts itself to dependency analysis. In this connection, however, it is a modeling device of great beauty, elegance, and pregnancy for

representing many psychological theories and for putting these to empirical test. The importance of Q-technique lies more in these psychological applications than in any of the statistical devices it employs or represents; and it would mean little or nothing if it were divorced from the methodological and psychological matters with which it is associated and in terms of which it gathers its meaning. We repeat that its affinities are with logical analysis and experimental design, as much as with factorial and variance analysis; and, indeed, it owes a great deal to the methodological essays of Egon Brunswik (35), to the writings of Feigl, Kaufmann, and others in this country, of Wittgenstein, Schlick, and others elsewhere, and to the analyses of J. R. Kantor (88, 90, 91).

PROTASIS

We said earlier that *psychometry* has long had its own methodology. Spearman (148), Burt (41), Thomson (181), Thurstone (188), Cattell (48), Babington-Smith (12), and many others are basically rooted, in their work, in R-methodology (167). With the notable exception of Burt (41), none of these psychologists has given any attention to the methodological implications of his objectives and of the techniques which subserve them. Ward (197), alone, reached in a penetrating manner into R and was highly critical, as a consequence, of its assumptions. We shall attend to these matters as we proceed and shall find ourselves in agreement with Ward. It will be seen that R has technological rather than psychological implications. On the other hand, Q is of concern to psychology in its most general aspects.

Compared with R, Q has a very brief history. Its principles have to be developed *de novo*. Critics of the last fifteen years altogether missed the methodological implications of R and consequently the different possibilities of Q. For this reason our own early papers could not be convincing. In the meantime, logical analysis is better appreciated, and Fisher's great work on the design of experiments is ready, now, for the picking, as it was not in 1938. We are able to present a case, therefore, for the single case, which is now reasonably complete in its essentials and which should be of some service to psychology. We proceed, therefore, toward a naturalistic study of behavior.

CHAPTER II

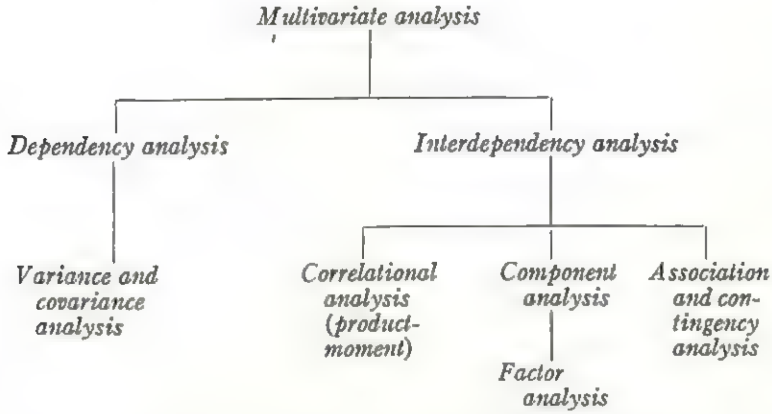
DEPENDENCY FACTOR ANALYSIS

FACTOR ANALYSIS

FACTOR analysis is a complicated subject. Traditionally, its object was to discover "unitary" traits or "elements of the mind." Great things were expected of these discoveries, but nowadays little but descriptive or incidental significance can be attached to them. It has been mentioned that two different methodologies are associated with factor analysis, one R and the other Q.

The area of inquiry, in both R and Q, concerns the analysis of complex, multivariate situations, that is, in which several possible effects and the like are at issue; their analysis along statistical lines is called, quite generally, "multivariate analysis." But there are two main divisions of the area, called "dependency" and "interdependency" forms of analysis, respectively, to which some reference has already been made. The former corresponds to what is widely regarded as *the* scientific method *in esse*, in which there are clearly formulated dependent and independent variables. In interdependency analysis, on the contrary, there are no such a prioristic formulations, and all the possible variables have an equal status, the problem being that of discovering relationships between them *de novo*. The typical viewpoint about these issues is well illustrated in Woodworth's Introduction to his *Experimental Psychology* (206) where he has precisely these distinctions to make between dependency experimenting, on the one hand, and correlational studies, on the other, which he and everyone else up to now have always regarded as typically a matter of interdependency analysis. In statistical theory, however, *variance analysis* and all Fisher's experimental methods are regarded as within the framework of dependency analysis, and correlational theory and the methods of association and contingency are held as certainly to be matters of interdependency analysis. Kendall (93), likewise, has pursued this line of thought, and

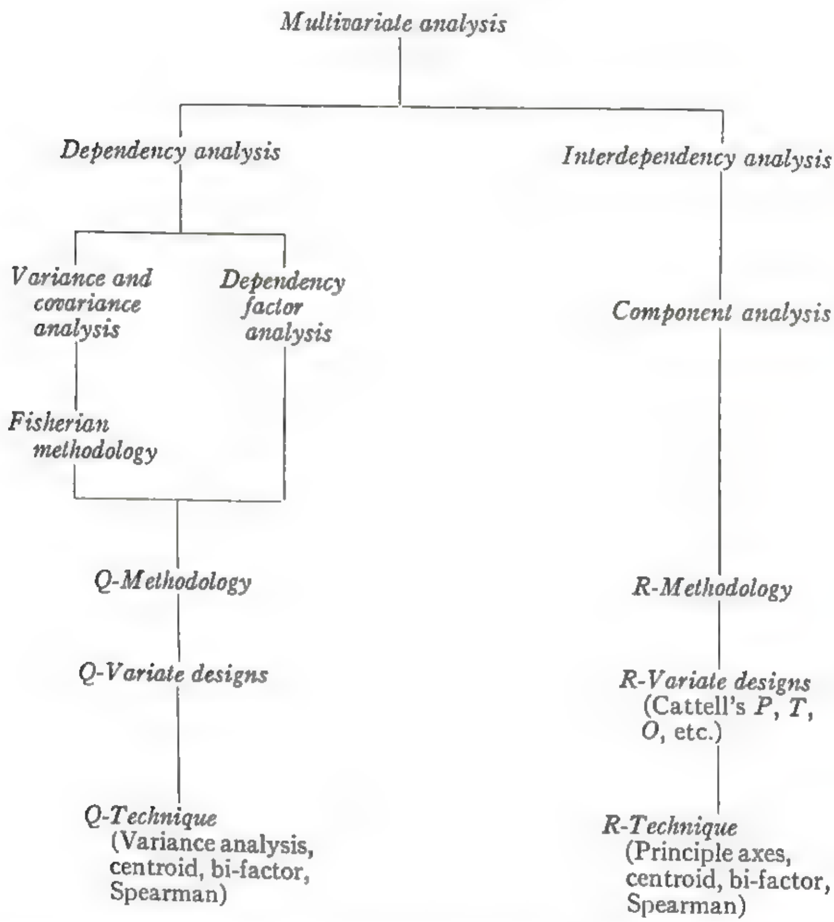
makes the issues very clear by representing them on a sort of genealogical tree, the main features of which, for our present purpose, are as follows:



What is meant by "component analysis" need not concern us unduly, but it is an important distinction, made by Kendall, between the more purely statistical aspects of what psychologists call "factor analysis" and factor analysis itself, which is regarded by Kendall as a special case of component analysis. Otherwise the tree merely makes clear what, up to very recently, was the obvious state of affairs with respect to the place of correlational methods and factor analysis in the multivariate domain.

Our own thesis is that factor analysis can also be employed for dependency analysis. It is not denied that there might be something analogous to component analysis, as Kendall supposes, under the rubric of interdependency analysis: it is certain that R-technique has been so regarded up to now. But Q-technique belongs essentially to the *dependency* branch and not to the collateral line. Thus our theme may be put in Kendall's tree as shown on page 32. One of the essential purposes of these chapters is indicated in this little "tree," and the theme will reappear, as we proceed, in different guises. We note that Q-method involves the joining of two methods of dependency analysis, one Fisher's and the other a reformulation of factor analysis which we are now to introduce. Fisher's methodology is well known, but we are to employ it in a rather special way, for the *structuring* of the samples we use in Q-technique. The matter is dealt

with in chapter iv. The structure is used to deal with *explanations* or the *theories* at issue. Dependency factor analysis, on the other hand, is employed for putting experimental propositions to test. There can be no rules of a routine kind for this; it consists, in practice, in solving the centroid factors, by rotations, so as to provide answers for



propositions which have been asserted beforehand or which are "held" theoretically. There are almost as many rules for this as there are problems to solve.

There is need, however, for some more general discussion of factor analysis in relation to Q-methodology, so as to place our thesis in proper context.

CONSPECTUS OF FACTOR ANALYSIS

That confusion abounds in factor analysis is almost self-evident. "Spearman's own contributions," Kendall (93) has said, "if not widely accepted, were at least intelligible; but since his time the wood has become a jungle into which few statisticians care to venture" (p. 60). That a dark impenetrability exists is only too true. Distinctions have to be drawn between the *methods* (such as Hotelling's, the centroid and the bi-factor), and the techniques, such as R and Q, as well as their broader bases as methodologies. Statisticians much prefer the method of principal axes, and psychologists the centroid method. Thurstone (188) *rotates*, but Burt (41) does not do so, for precisely the same centroid factors. Stephenson and Burt (44) have long differed about R and Q. Whatever is done, by whatever method and in whatever technique, no one knows when to stop factoring (196), and different ways of estimating factors have been described, which scarcely anyone understands (181). The method of principal axes provides orthogonal factors, whereas Thurstone uses correlated factors, extended vectors, and the like. The techniques are rapidly proliferating, or so it would seem, for, in place of the two independent *systems* R and Q defined in 1935 (171), there are currently appearing many additional letters of the alphabet (*P*, *T*, *O*, and the like, of Cattell, 48, 50). But the latter merely adumbrate *variate designs*, and only R and Q have the status of methodologies. It is only too easy for the unwary, however, to place these techniques *P*, *T*, *O*, etc., on a par with the basic systems R and Q. In these circumstances it may well seem that little can be trusted, much less understood, about factor analysis.

Intelligibility can be restored, however, by paying proper regard to the logic of scientific method. When this is accomplished, simple factor methods are all that we need for experimental purposes, and anyone who finds a *t*-test or an *F*-test not unsavory or confusing will be able to use factor analysis quite confidently. No dependency analysis need ever take more than a few hours for its solution, and not weeks, months, or years. First, however, we require some understanding of interdependency analysis.

INTERDEPENDENCY ANALYSIS

By "interdependency analysis" is meant that form of multivariate analysis in which it is *not* possible to regard some variables as independent and others as dependent effects; none, in short, is picked out on any prior grounds for special regard. The early factorist contemplated the possibility of universal factors, such as Spearman's *g*, as the natural extraction of such interdependency analysis, squeezed out by an inductive methodology in its purest and most objective form. The concern is with inductive inference and not merely with descriptive statistics.

Typical of such R-technique studies is a recent one concerning intelligence (180), in which five centroid factors were obtained at the outset, which were rotated in pairs, yielding five orthogonal factors. Each was given an explanation, a posteriori, which largely consists of attaching to the factors the names that distinguished the *tests* at the outset—a test involving mathematics gives rise to a factor which is called a "mathematics factor." Nowadays, however, analysis is likely to be pursued in terms of correlated factors, and the author of the paper proceeded to make a reanalysis in terms of correlated factors. The result was eight factors: each of five obliques was complex, composed of four second-order factors and a part specific to each. Three second-order factors were necessary to account for the correlations between the primary oblique factors; and the investigator was driven to conclude that these second-orders must be complex as well. He finally asked what had been gained by this analysis? The results are in agreement, he concluded, with the view that factors are "not all unitary" but are built up in the individual mind by the external influences of neighborhood, school, books, tools, and the like. More and more analysis, then, should yield more and more factors, progressing toward "unitaries" as the ultimate object. The author, however, had some doubts at this point and supposed that a time comes when a halt has to be made to this never ending analysis: he questioned whether he was justified in going beyond the original five orthogonal factors.

Except to say that if too many factors are contemplated, matters verge upon the error plane, the author does not pursue his interesting doubts further. But we would like to do so, by asking what *theoretical*

matters are involved. The redesignation of the factors is hypothetical, but scarcely theoretical. Otherwise it seems that two matters are at issue: (*a*) that unitary factors do, or do not, exist and (*b*) that the psychological beach is strewn with many pebbles; that is, that intelligence is to be regarded as the consequence of innumerable opportunities and experiences, the accessibility of books, tools, and all else. Now it is always difficult to assert, merely *ex cathedra*, what is and what is not a valid scientific question and a proper theoretical approach. But our own belief is that it is not too far wrong to suppose that both *a* and *b* are pseudo-hypothetical only, as is the explanation of the factors in the first place. Certainly, they are irrelevant to any proper scientific regard of intelligent behavior. Really, no theoretical standpoint, in any psychological sense, is anywhere at issue in such a study. Indeed, the R-factorists have often taken pride in having freed themselves from "theory."

Attention has already been drawn to the fact that Kendall (93), as well as factorists generally, place factor analysis squarely, and without question, in the domain of interdependency analysis. Given a set of n observations for each of p variates, the interdependency problem, as the statistician sees it, is (i) can m new variables be found, linearly related to the p, but fewer in number, which will account for the original variates, and (ii) what are these m variables? The remarkable subsumptive power of the correlation technique is of central importance: the $n \times p$ observations are reduced to a $p \times p$ matrix of correlation coefficients, containing only $\frac{1}{2}\,p\,(p-1)$ coefficients, i.e., excluding the diagonals. This subsumption is continued, in factor analysis, by developing the fundamental theorem that the p-variation can be expressed in m ($\leq p$) dimensions if and only if the matrix has rank m. Thus, if there are 1,000 observations for each of 10 variates, the 10,000 readings are reduced to only 45 correlation coefficients, and, if these can be subsumed still further by *one* factor, only *one* explanatory principle need be involved for all the 10,000 observations. When the data do not admit of subsumption in fewer than p factors, no mathematical solution can provide the new variables, and statistical methods are invoked, the most reasonable being to find those along which variation is maximized, which is what Hotelling's method of *principal axes* achieves (82). As Kendall puts it:

> This approach is, I think, the first which the statistician would consider, and he may be a little surprised to find that the psychologists by no means give it pride of place, particularly as it has properties which other methods do not possess [93, p. 63].

As is well known, the method provides orthogonal factors. But as for any of the other factor methods, there is no proof that a specified number of factors is enough to account for the original variation. The method, of course, is almost useless for our postulatory-dependency purposes.

The *centroid* method is also within the purview of interdependency analysis, but it gives quite a different solution. It considers the situation for p points in n dimensions, and not, as in the method of *principal axes*, n points in p dimensions. It is implied that p will be small and n large; but the theorems now reached are entirely *permissive. Orthogonal sets of factors can be produced, for this method, in innumerable ways*—which, of course, is what makes the method of *rotation* (Thurstone, 188) feasible. Thurstone observes, correctly, that the initial solution provided by the centroid calculations is only one of these innumerable possible solutions open to our regard. Burt (41) accepts the initial centroid solution, which is mathematically quite arbitrary: it is hard to believe that Natural Kinds emerge this way. Thurstone, however, proceeds to rotate axes, with the object, as he says, of finding "meaningful," psychological, solutions. But this savors of being arbitrary, too, to the objective purist—it is easy to be wise after the event, and explanations are not usually too difficult to adumbrate a posteriori. Thurstone has therefore developed the conception of *simple structure* as a way out of a certain arbitrariness and the complete permissiveness of the centroid solutions. There is no doubt that Thurstone could never be satisfied with solutions provided by the method of *principal axes*, for these are purely of a statistical nature; his essential need was to "find" psychologically meaningful factors.

The principles of *simple structure* are not well understood. Logically and without reference to factorial designs (Fisherian), such structure means that, given factors α, β, γ, . . . , it is easier to concern one's self with their explanation when each variate has zero loadings in most of the factors—the "meanings" are then sharply focused. For Thurstone, however, *simple structure* is also objective: it is the

way the data *are*, a pattern of *interdependency* which can be discovered (if it exists) and which remains fixed, qua the interrelationships involved. This structure, however, is not always achieved, and certainly only rarely for orthogonal factors; and correlated factors are now the rule rather than the exception. The outcome is that (i) the number of factors proliferates (as Sutherland's study illustrates [180]); (ii) the tendency is for primaries to be the original variates all over again and not essentially explanatory *principles;* (iii) second-order factors now assume a large proportion of the initial variance, much of which is left unexplained; (iv) the compound variates tend to be neglected. In principle, the high-order factors might be explained in terms of the correlation of the primaries, so that the primaries are basic explanatory principles, as Thurstone seems to assume (188), or else they might have explanations *de novo* not covered by the primaries, as in Rimoldi's (125) study. In the latter case one wonders what, therefore, is of main importance, the primaries or the high-orders. Vernon (196) has remarked that the situation is a return to Spearman's "theory" that each test involves a *general* and a *specific* factor: Thurstone's *primaries* are comparable to Spearman's *specifics*, and high-orders are methodologically like Spearman's *general* factors. Little is known, however, about the pertinency of the primaries relative to high-order factors for testing in education and elsewhere. Clearly, some of the second-orders represent *main* effects in the factorial design sense, and the primaries represent "levels" for these.

Be this as it may, in the centroid method not only is the number of factors undetermined, but these can be rotated at will, providing a state of affairs bordering, for the statistician, upon complete chaos. There is again no sampling theory to tell us how many factors to extract. Thurstone's procedures are necessarily and basically those of interdependency analysis and not postulatory dependency, as in Q-technique. There is much in the statement that he has to discover structure, whereas we *postulate* it to answer theoretical questions. Time alone will say which goes farthest.[1]

1. It is clear, however, that what Thurstone describes as *simple structure* is the counterpart, in variance design, of confounded or other complex designs for structured samples, in his case for samples of *persons*, a topic dealt with in chap. iv. The explanations offered for his various factors are imputations placed upon the persons, and they

CONDITIONS FOR DEPENDENCY ANALYSIS

Apart from investing *simple structure* with a little mystery up to now, its factorists have, of course, stressed the *inductive* features of their methods. Views about the inductive method, however, are not so naïve as they used to be. Keynes, in his *Treatise on Probability*, raised serious doubts about the validity of inductive inference in correlational theory: sensible investigators, he suggested, "only employ the correlation coefficient to test or confirm conclusions at which they have arrived on other grounds." The logical analysts explain away induction altogether, or else search for an inductive logic *de novo* (46). The psychologists Kantor (90) and Brunswik (35), in the vanguard of modern methodology, however, have made important contributions to the matters in which we are interested. Kantor provides the basic postulate that scientific behavior is concrete inferential interbehavior. No absolutist deductive or inductive methodology is ever at issue; instead, every scientific decision (92), in principle, is to be regarded as one involving its own rules; and certainly no single set of procedures should be expected to fit all the inferential settings of science. Similarly, Brunswik (34) remarks that thinking is behavior in which "cues" are used in a somewhat "crudely machine-like fashion"; and scientific thinking has the same look about it.

This, to us, makes good sense. In Kantor's methodology it reserves a key place for reality. In its terms scientific method is never merely a closed system of definitions, axioms, and postulates, from which theorems are derived, ready for empirical or credibility tests. Rather, the scientist has a large repertoire of modes of inferential

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could be represented in balanced or other factorial designs for samples of persons along the lines of, say, Table 1 of chap. iv. That is, Thurstone sets out to discover, naïvely, what in principle could be postulated *beforehand* if one were prepared to adopt a *theoretical* approach. There is a difficulty, however. Psychological postulates cannot be asserted for samples of persons, at least not with the ease with which this can be done for samples of statements and the like of Q-methodology. A practical dependency analysis is thus difficult to achieve in R. Theories of the kind that psychologists should be interested in cannot be represented in samples of *persons*, and *proofs* cannot be offered of the explanations given to factors in R. Of course the *tests* in R-technique, corresponding to *variate designs* in Q-technique, offer great scope for operations; but here again these occur only under narrow conditions, for the "rule of the single variable," i.e., for the measurement of one attribute at a time.

behavior ("cues") which he brings to bear upon the problems he investigates, often doing so in a very "crude" manner. It is with such a repertoire that we approach dependency factor analysis. It seems to us that not only can many arguments and conclusions in psychology be represented as main *effects*, for at least *two* levels, but that there are many ancillary tricks of the experimenter's trade open to us. Many examples in which theories, arguments, conclusions, and the like are formally represented in Q will be given in the following pages. But, in addition, we make use of various "cues" to govern our analyses, more especially for the factor analysis of unstructured samples. Thus, in a Q-study in which clinical experts and novices participate, we may assert that a solution will be looked for which gives more factors to the clinicians than to the novices, on the ground that it would otherwise be difficult to explain what the experts do in terms of factors for novices. Or, again, if persons A, B, C, and D can be shown, sociometrically, to be linked to another, E, the investigator may have a "hunch" that factor solutions centered upon E, rather than upon A, B, C, or D, will prove pregnant in some way. There are countless "cues" or "tricks of the trade" of this kind in every science. All are deliberate impositions of inferential and empirical possibilities upon otherwise neutral situations.

Thus the time has come to give greater freedom and scope to the investigator, as we are seeking to do in the postulatory-dependency methodology, on the ground that, after all, this comports best with the way scientists really work. The false objectivity of interdependency analysis and the exaggerated virtues of inductive method must be seen as clichés with little to commend them. The centroid factor method leaves open for us innumerable possible solutions, and the concreteness of inferential interbehavior (see below) contemplates, no less, innumerable possibilities in the pursuit of scientific investigations: it is difficult, therefore, to imagine a better *modus operandi* than these two, together, make possible. Dependency factor analysis is merely the *modus operandi* fully exemplified. It consists of rotating centroid factors to reach predicated effects, if the data can provide them, by using every applicable "cue," "hunch," or "trick of the trade" to guide us. The investigator puts his questions to the data, as testable propositions.

We have a long list of "tricks" at our command in relation to dependency analysis. Chief, perhaps, is the practical one of not analyzing data *too much:* it is all too easy to "see through" reality, as C. S. Lewis put it (107), but never to see it as it really is; to "see through" a garden to its soil and bacteria and miss the surface beauty. Similarly, in factor analysis we now have many instances on record of data having been analyzed most precisely into many correlated factors, with primaries and high-order factors apparent, while the *obvious* is completely missed. Thus we usually "make do" with fewer factors than others might employ, on the grounds that, although the data might hold more, we are satisfied to find some empirical proof for propositions asserted beforehand—whatever *else* may also be there. We proceed from the more obvious to the lesser, however, on theoretical grounds, and in every study care is needed about the inherent necessities. Thus, when Cattell (51) studied women on a college campus with respect to temperamental traits, in order to achieve apparent objectivity the appraisals were made about the women by *others* from the "outer frame of reference." The correlations were then analyzed by Cattell *from the standpoint of the women rated:* we would analyze them instead *from the standpoint of the persons who did the ratings*, because theirs are the actual operations at issue.

The precise form of analysis to which data are subjected, however, depends at bottom upon our conception of scientific method as such. Obviously, there are basic elements and rules of procedure (92) which we fully accept. But the logical analysts and logicians (except for J. R. Kantor, 90) overlook an axiom to which, with Kantor, we attribute greatest importance. It is to the effect that all scientific behavior is *concrete inferential interbehavior*, that is, relatively specific to each experimental situation. This means that there are no absolutist deductive, hypothetico-deductive, or inductive methods or powers at issue. Every experiment, rather, requires its own rules, or some specific to it; and no single set of procedures can fit all the inferential interbehavioral settings of science. Thus we are careful to regard the postulatory-dependency methodology of Q as "open-ended"—the precise form of the analysis undertaken is determined by the experimental situation.

By happy chance, the *permissiveness* of the *centroid* solution in

factor analysis is precisely what one requires for the doctrine of the concreteness of inferential behavior in experimentation. The statistician finds nothing to favor in the permissiveness of the centroid method and prefers the certain and constrained solutions provided by the method of principal axes. But for us the opposite is true—there could not have been a better device, for our purposes, than Thurstone's geometrical rotations for centroid factors, and we make much use of the method in our factor studies.

Thurstone, as we have seen, seeks to limit the permissiveness of the centroid method by searching for *simple structure* in the data. If this is regarded as a matter fundamentally of *interdependency* analysis, as it could be, we believe this must break the axiom of the concreteness of inferential interbehavior. In short, it is difficult to accept one kind of geometrical substructure as, in principle, the only basis for inferences. In practice, however, by way of single-plane and other solutions to the rotational problem, Thurstone's procedures have probably far more latitude than might appear on the surface.

In our own case the rotations we pursue follow two broad principles. For unstructured samples we seek to determine sometimes what orthogonal structure best fits the data, for a balanced block design of effects, usually for two levels each (representing *positive* and *negative* loadings, respectively, on the factor). A balanced block design is called a case of "simplest structure," to distinguish it from Thurstone's concept of "simple structure." Ours are always orthogonal, but attention is also given to some properties of the structure which are widely overlooked in multiple-factor analysis. We not only seek to "explain" factors α, β, γ, . . . , but we also ask that all possible combinations of the factors, such as $\alpha\beta$, $\beta\gamma$, . . . , $\alpha\beta\gamma$, . . . , should be explained. The *interpretative power* of a factor rests in the combinations it helps to explain, as distinct from its *analytic power*, which concerns the explanation it provides, in Thurstone's sense, vis-à-vis a "primary" factor. We shall make further reference to these matters in later chapters.

Usually, however, attention is focused upon the variate designs and the problems that these set out to solve. We rotate, therefore, to see whether there are solutions of the expected or predicted kind. Thus, in a study of experts and novices, we suppose that experts should provide more factors than novices, and we rotate to achieve

this (if it is a possible solution). Rotation to dependent effects is something of an art. We would describe an art, however, not as non-scientific but merely as an indication of the fact that many "tricks of the trade" are involved, with which we have to become familiar.

PROPOSITIONS AND PROPOSITIONAL SETS

Against this background certain conditions under which dependency analysis is conducted require mention. These concern the matter of general, singular, and testable *propositions* or *propositional sets*. They represent the problems we have under investigation and the predictions we make with respect to them.

There is now an extensive literature in the philosophy of science about propositions, a neglected topic since the days of Aristotle, with important contributions by Wittgenstein (203), Schlick (139, 140), Braithwaite (32), and others. Again, however, we have to try to grasp what is essential for practical purposes, and for these Kaufmann's (92) treatment of the subject seems to us to be eminently satisfactory. Briefly, a statement of the kind "All crows are black" is a general proposition. To say that "A crow is black" is clearly singular, but not testable. When, however, we can point to a particular crow X and assert that *it* is black, a singular testable proposition is at issue. We shall elaborate upon these matters later. Meanwhile, it is sufficient to say that in empirical science singular propositions are put to test and, clearly, never general ones. We can never prove that *all* crows, for example, are black. Several sources of confusion met, head-on, in psychology with respect to general propositions. First, they were confused with *theories*. Second, they were employed as basic assumptions in R-methodology. To make matters worse, it was thought that one could prove theories as general propositions. We shall see in due course something of the devastating effect of these mistakes. All psychometry and the techniques based on *individual differences* purport to deal with general propositions and to test theories as general propositions. Instead, experiments can be conducted, if at all, only in relation to singular propositions.

Thus our approach in Q is to test theories by way of the singular propositions that may be derived from them. We shall use Jung's theory of personality, later, to exemplify these matters. Jung's theory did not suppose or require the assumption that something

called "introversion-extroversion" might exist and that everyone must have it in some degree. Instead, a complex matter was at issue, concerning behavior. Given the theory, we seek to represent it in Q-method, and then test anyone we like, to answer a question of the kind—Is X habitually introverted or extroverted? In the past it was thought that one must have a validated scale of some kind in order to answer such a question by making a measurement. Instead, the question can be answered without any such scale, merely in relation to theoretical issues.

But to return to propositions. These concern us, in Q-methodology, at two places—first, in relation to *samples* and, second, in relation to *variate designs*. In any factor study the concern is with a sample (n) of items which is quantified, each array of scores for the sample constituting a variable or variate. A particular study may involve several variates, say M in number. Thus we might pursue a study, along Q-technique lines, to compare the diagnostic skill, or lack of it, of three clinical psychologists, A, B, and C; three students, D, E, and F; and three control subjects or total novices, G, H, and I—nine persons in all. A sample of n statements of some kind would be used, and there might be many variates for the nine experimental subjects, all directed, perhaps, upon the diagnosis of a particular case, X, under various conditions. We predict that A, B, and C will have some factors particular to them, different from those for D, E, and F or G, H, and I. Several propositions of the kind can usually be asserted about the variates for any such study. Thus, we might expect a factor common to D, E, F, G, H, and I to represent a common-sense diagnosis. Or we may assert that stereotypical diagnosis, or "psychologizing," will be distinguishable as a factor for the clinicians; also, that a basically sound diagnosis can be made by these same experts. All such propositions can be tested without regard to any *reasons* as such for the facts. Hunches, guesses, and the like may well be at issue, which we are far from despising. But we also like, of course, to be able to say *why* these assertions are made; rather, we wish to give *reasons* for the assertions where we can. Indeed, if we have a theory, we presumably made some original predictions and asserted some of the variate propositions in relation to this theory.

One of our achievements has been to represent such theory,

formally, as *structure* in the samples we use in Q-methodology. The *reasons* for the propositions, in the profound sense, are embodied in the samples. The structure is a matter for Fisherian balanced designs. With respect to these, we can assert propositions, too, quite different from those dealt with by the variate designs. These concern the actual *explanations* or theory for such factors as we may reach. Thus we are always concerned in Q with two kinds of facts and two regions of propositions. The one concerns the *variates* and factor analysis; the other concerns the *samples* and variance analysis.

It is possible only to state these issues briefly at this point, as a basis for development in the pages and chapters to follow; and the reader must bear with us for awhile. Dependency analysis involves the assertion of these various propositions. But it is also rather more. The question of "hunches" and "cues" has been referred to already, and we wish to mention it again. In the example just mentioned we would regard it as a valid scientific procedure to assert the proposition that the experts A, B, and C must provide *more* factors, in the Q-study, than either the students D, E, and F, or the novices G, H, and I. This we are prepared to justify, indeed, in terms of the probabilities of the rival possibilities at issue, along the lines of the Bayes-Jeffreys theorem. We wish to support all such "cues" or "tricks." But there is a more important way in which we wish to accept the use of a theory. Everyone in psychology knows what is meant by "apperception." The botanist, walking through the wood, will at once notice the purple hellebore or the stitchwort, whereas his friend, a professional baseball player, will miss them. But the player will see balls and bats in the stones and branches of the wayside. Many theories in the social sciences, and in psychology in particular, give rise to just such an apperceptiveness. Theory can be complex, like the total background of a botanist's or a baseball player's experience in the special fields concerned. It leads the psychologist, therefore, to be apperceptive for certain facts, that is, really, to know what to look for. So-called "psychological intuition" is no doubt something of the kind. In any case we have to reserve a place for such perspicacities in scientific work, and we do so in factor analysis by permitting the psychologist to rotate factors with his theoretical expectancies in mind. He knows what to look for and can be relied upon to observe facts that a routine method of analysis could miss

altogether. However, subjectivity is not involved. For all factors so reached are capable of verification or proof. The botanist finds the hellebore; but the baseball player can also see it when it is pointed out to him. The same is true of the factors we deal with in Q.

Dependency factor analysis, then, is concerned with propositions about variates and samples and with the use of various devices in order to reach solutions to the centroid factors, including the holding of theories in mind by the investigator as he rotates his factors. The matter can be exemplified, rather than receive any short definition. But where, it will be asked, are the independent and the dependent variables? The former are in the *sample* and in the conditions of instruction or the like, on the basis of which the sample is quantified. Each variate, on the other hand, is a *dependent* variable. Factors, likewise, are thus dependent variables.

For practical purposes we find it necessary to refer to three kinds of propositions with respect to variate designs and variates. They are as follows: (1) "general theoretic" propositions, (2) testable propositions, (3) induced or operational propositions. Problems suggested by a theory, or which are relevant to it, are usually stated first in general theoretic terms. Thus, in relation to psychoanalytic theory, we may assert the theoretic proposition that dreaming is an unconscious form of wishful thinking. This would usually be elaborated upon, so as to define what is to be regarded as wishful. A particular dream of a particular person, X, would then be made the subject of the singular proposition: This dream of X involves wishful thinking. Its testing can then be pursued, as it happens, along Q-technique lines. X would be called upon to perform several Q-sorts, under different conditions of instruction, upon a sample of statements, the whole being designed to have unconscious reference to X's wishful thinking, if it occurs in relation to his dream. About the variate design, so put together, it would be possible to assert perhaps several singular, and testable, propositions—such that a factor f_1 will be present which will be explicable as X's wishful thinking in his dream. Many examples of such testable propositions and of the more general theoretic ones preceding them will be given later. Sometimes the testable propositions may almost seem to be a priori rules of procedure, as when, in a study of expert clinicians versus novices, we assert that the former will have more factors than the latter. The

general theoretic and the testable propositions are asserted at the outset, before the experiment begins—they are the *raison d'être* of the variate design. But sometimes a specific assertion cannot be made until data are being analyzed, after the experiment is completed: that is, the facts will indicate, at times, what the specific form of a proposition *should* have been, although that something of the kind was expected and predicted was stated at the outset. The precise form of the proposition, in the latter case, depends on certain facts that can be available only as a result of the experiment: such are *induced* propositions.

CONCLUSION

These, then, are some of the principles of dependency analysis upon which Q-methodology is based. At bottom, our proposals depend upon a belief that scientific behavior is concrete (Kantor, 90), and never the object of any absolute principles of deduction or induction. It is for this reason that we stress the lack of any routine procedures in dependency factor analysis, such as are involved in searching for *simple structure*. The distinction, likewise, between theories which are never tested for their general implications and singular testable propositions may seem innocuous; but, because the matters were confused, theories were never tested in R. Their separation, on the other hand, has proved to be richly rewarding in Q.

CHAPTER III

DIFFERENTIATION OF R AND Q

INTRODUCTION

READERS of Plato may remember that the comic poet, Aristophanes, divided mankind into three sexes, and not two: there was male, female, and a combination of the two which could look both ways and walk backward or forward as it pleased. When in a hurry it rolled. This man-woman, however, was a child of the moon. The idea that R- and Q-techniques are merely different ways of looking at the same facts seems to have been plagued by this same lunar influence. It has been difficult to convince anyone that the two are necessarily distinct in essential respects. It is perhaps always difficult for those living with old ideas to make adjustments very quickly, as Brunswik, in connection with R and Q, found out for himself (35). A summary of some of the principles at issue may help, however, to hasten matters for the reader.

A lengthy interchange of views about the pros and cons of R and Q was conducted, during 1935–38, by Sir C. Burt and the present writer. The outcome was our joint paper on "Alternate Views on the Correlation of Persons" (44). Burt later expanded his standpoint very considerably, in his *Factors of the Mind* (41). Basically, Burt and statisticians who looked at these matters (93) were fascinated by the *interdependency* analysis of data. Even Spearman had been intrigued by the possibility of discovering a *structure* (*g*) in everyone in some degree, as a vast inductive matter, issuing *necessarily* from interdependency analysis in R-technique. Burt's excursions into the metaphysics of factors, into Keynesian realms of "atomic unity" and Broadian "blobs," and into quanta theory (41) were clearly rooted in such interdependency analysis. Believing this, he felt that it was obvious that R and Q could scarcely deal with different matters in the last analysis; so Burt was confident that, in the end, proof would be provided for R and Q as different ways of reaching the same results.

ASSUMPTIONS ABOUT INDIVIDUAL DIFFERENCES

Underlying R, however, there were postulates about individual differences which the present writer could not accept. When a mental test is applied to a sample of persons, each person gains a score. These comprise the individual differences. No doubt they represent facts of some kind. When three persons, A, B, and C, gain scores x, y, and z, respectively, and $x > y > z$, the *transitory postulate* (Keynes, 94) is at issue, upon which all else in correlational theory and factor analysis depends. This is to the effect that when $x > y$, $y > z$, and therefore $x > z$ are all acceptable, a certain "significance" is at issue (93). This "significance" concerns, in R, some protopostulatory beliefs about *abilities*, *potentialities*, or the like: only on grounds of such belief could one accept the transitory postulate for test scores. Already, therefore, there is a root of subjectivity in R-methodology. Without the transitory postulate, no mental test scores, or any others, could be correlated with any justification or meaning. In R the postulate is rarely questioned. Yet it is sometimes scarcely a warranted assumption to make. There are often doubts, for example, as to whether children who gain the same score in a test, gain it for the same reasons.

It should be noted that the correlation technique can apply to data other than for individual differences, although this has sometimes been doubted (181). One of the commonest objections to Q, indeed, is that correlation coefficients cannot be calculated for one person only. The method of correlating data applies, of course, to any data for which the transitory postulate is a warranted assumption (94). Thus, when we invite a person X to rank a suitable set of works of art, from the one he likes best to the one he likes least, or to perform a Q-sort upon them, the array is ready for correlating with others if the transitory postulate is an acceptable assumption for it and the others. *Intra-individual* "significance" is essential for the works of art, relative to X. This can be more readily assumed, since X is a particular case, than is ever possible for the postulate in relation to individual differences. The situation is under greater control for the single case. It has merely to be assumed that the principle is consistent in *his* case, whatever its nature or for whatever reason. Naturally, there are ways of insuring that the assumption is warranted or reasonably acceptable—these have to do with the homo-

geneity of the sample of works of art and with our instructions to X. The conditions can be assured for each person in turn. But it is nowhere a necessary assumption that any transitory principle applies to what X does relative to what any *other* person does. That is, it is never necessary to assume that X is doing the same kind of thing as Y, differing only in degree. We *prove* whether or not something of the kind may be the case, by way of factor studies. The transitory principle, then, is of fundamental importance, as is the concept of "significance" underlying it—yet at least one of our critics has regarded these issues, apparently, as of no consequence (50). The advantages with respect to it certainly lie with Q rather than with R.

Again, we believe that serious mistakes were made in differential psychology (meaning by this the technology of individual differences) when it was applied to matters of more essential concern to *general* psychology. Rational laws or theories come within the purview of general theoretical psychology. When the R-factorist tried to deal with such theories, he ran into several difficulties. First, he was involved in trying to prove or disprove theories for their *general implications;* and, to do so, he had to suppose, further, that everyone must have every attribute to some degree. With respect to general implications we are now wiser. It is necessary merely to provide a single exception to a general proposition to dispute it. Thus, by way of individual differences, Spearman (148) wished to prove that every mental test would involve two factors, one general to all tests (g) and the other specific to each test itself (s). The present author spent much time testing the proposition so asserted (159). But as naturally as there are white crows, on occasion, so there are tests which must break the rule for two such factors as g and s (even if the rule really exists, just as black crows certainly do). It was a simple matter indeed to find such tests. Thus the initial proposition was incapable of proof. When the present writer argued that all rules have exceptions, he was admonished for "purifying" the data to suit the theory (159). Instead of this, we were using the theory to help us to discover *other* facts *as well*—the factors discovered as exceptions to g and s factors were precisely those rediscovered later, for example, in the laboratories of a Thurstone or a Cattell. It was easy, however, for Burt (41) to replace the *two-factor* proposition by a *four-factor* one; Thurstone (188) put forward a *multiple-factor* proposition; and Thomson (181)

as readily replaced it by a *sampling* "theory." Yet what had originally been at issue was a theory not about factors but about *noesis* and the ultimate nature of cognition and intelligence. The factor *g* merely represented the *noetic* principles, and all degrees of *s* were reserved for *anoetic* principles of all kinds (149). These were matters for testing, if at all, only under singular propositional conditions, that is, as matters within the purview of theoretical science. A single person, *any* in principle, could have served as a subject for the essential investigations. The singular problems, however, were never even considered, much less investigated, by Spearman or anyone else. Instead, it was supposed that the theory had general implications which were representable by way of individual differences, to be studied or, rather, proved or disproved, once and for all, by using the theorem of two factors for *g* and *s*. Science does not work in this way, however. In principle, theories are never testable for such general implications.

There were other serious limitations in R-methodology and its foundation in individual differences. Every person tested in R gains a score of some degree for every attribute or factor at issue. The assumption is that everyone is so testable, in some degree, for every attribute. Everyone may be intelligent, for example, in some degree. The same applies, it would seem, to attributes of temperament, such as cheerfulness, activity, or serenity. But not all persons have blue eyes in some degree. Nor can the assumption be maintained by supposing that *zero* amount may be included as a matter of degree: brown-eyed Bess does not have zero degree of blue in her eyes *in any operational sense*. We cannot measure the knee jerks of a limbless sailor or agree that no knee jerk in an intact leg is the same thing as no knee jerk of a nonexistent limb. When one thinks of it, the same seems to be true of many, perhaps all, personality traits and of all behavior. Bess may lack serenity in the same sense that she lacks blue eyes or that a one-legged mendicant lacks a limb. But if Bess lacks serenity *where she should have it* or, in principle, where operational matters are at issue, *zero* amount of serenity is very different indeed from mere lack of it. The zero knee jerk of a paresis patient in a mental hospital has a very different significance in comparison with the lack of any knee jerk for a limbless sailor. These distinctions are clearly of great importance in principle and in practice. The assump-

tion of generality for all attributes is both erroneous and inescapable in R-methodology. It is in no way involved in Q, where, if Bess is limbless, we say so without confusing zero and none. *Lacking* in serenity would gain the central or neutral score in a Q-sort; having *no* serenity where much was to be expected would be given high significance, i.e., an extreme score in a Q-sort (either the highest or the lowest of the forced frequency distribution).

THE MATRICES OF DATA FOR R AND Q

In view of such considerations, therefore (and there are other difficulties about individual differences into which we cannot enter here), we proposed in 1936 to cut lose from them altogether as postulatory matters. We therefore wrote a paper for *Psychometrika* (171) called "Foundations of Psychometry: Four Factor Systems," which achieved this separation by defining two independent systems or matrices of data, one for R and the other for Q, upon which to base any discussion of the matters at issue. In R, individual differences, with all their assumptions, warranted or not, are basic to all else. In Q, intra-individual "significances" alone are postulatory, replacing the role of individual differences completely. These matters were clearly stated originally, but our critics Burt, Thomson, Cattell, and recently Babington-Smith (12) have continued to suppose that only one matrix is ever at issue, involving individual differences either directly, indirectly, or fundamentally, which, looked at *down* its columns is R, and *along* its rows is Q.

The four systems initially defined in the 1936 paper were as follows:

System 1.—(R). *Tests* are applied to a sample of persons, and the correlations between the tests are factored. Individual differences are at issue.

System 2.—(Q). *Persons* are applied to a "sample" of statements or the like, and the correlations between the person arrays are factored. Intra-individual "significance" is involved.

System 3.—The transpose of *1*. Data which have been standardized in *columns* for purposes of *1* are now standardized along the *rows*, and the correlations between *persons* are factored. (Cattell's *P*-technique is of this system.)

System 4.—The transpose of system *2*. Data which have been

standardized in *columns* for purposes of *2* are now standardized along the *rows*, and the correlations between "statements" are factored.

Thus, if we consider a simple case for R, the data for three tests, for three persons, may be as in Table 1. The table, of course, is for illustrative purposes only, and we could never use in practice so small a sample of persons in R. The persons represent a *sample* from a defined population of some kind; the tests are rarely so defined.

TABLE 1

Persons	Tests		
	a	*b*	*c*
A	100	3	0.50
B	75	7	0.93
C	50	5	0.65

TABLE 2

Persons	Tests			Means
	a	*b* (Raw Scores)	*c*	
A	$100x$	$3r$	0.50 mm.	?
B	$75x$	$7r$	0.95 mm.	?
C	$50x$	$5r$	0.65 mm.	?
Mean	$75x$	$5r$	0.70 mm.	
Standard deviation	$20\ 4x$	$1.6r$	0.19 mm.	

Test *a* may have consisted of canceling letters on a sheet; *b* of solving problems in arithmetic; and *c* of the accuracy with which two points can be discriminated on the hand. Thus different units are involved down the columns, and the data should have been written as in Table 2. The units are x, r, and mm., the first concerning a *letter* canceled, the second a *problem*, and the third *millimeters*. Clearly, we can add the data in any *column* and otherwise deal with it arithmetically, on the supposition that the units are the same and the transitory postulate is acceptably at issue. But the data cannot be dealt with by arithmetical means along the *rows*, since there are no postulates upon which to do so and the units are different.

R-technique thus begins with the columns. The units are first replaced by *standard scores*,[1] i.e., pure numbers, whose mean is zero and whose standard deviation is 1.0 per column. The scores for Table 2 thus transformed are as in Table 3. This reduction of raw scores to pure numbers is one of the most remarkable and simple devices in all psychometry, if not in all psychology, since it dispenses with multitudes of diverse units which would otherwise have to be contemplated. The data are now ready to correlate and to factor. But this can be done only with respect to the *columns*, since the coefficient requires standard scores for arrays. The scores along the *rows*, clearly, do not meet this requirement—their *means* are not zero, nor are their standard deviations 1.0, and only in very exceptional circumstances could they be so. Down the columns, however, these conditions are *necessarily* met.

TABLE 3

PERSONS	TESTS *a*	*b* (Standard Scores)	*c*
A	+1.22	−1.23	−1.05
B	0.00	0.00	+1.32
C	−1.22	+1.23	−0.26
Mean	0	0	0
Standard deviation	1.0	1.0	1.0

The correlations[2] between the *tests* for Table 3 are given in Table 4.

1. If X is a raw score, M the mean of the array (each column in the present case), and σ the standard deviation of the array, the transformation for X is given by $(X - M)/\sigma$. The transformation is automatically taken care of in calculating the product-moment correlation coefficient.

2. The product-moment correlation coefficient r_{ab} is the mean of the products of the pairs of standard scores for the two variates a and b, i.e.,

$$r_{ab} = \frac{\sum_{1}^{n} (s_a s_b)}{n} .$$

In this case

$$r_{ab} = \frac{-(1.22 \times 1.23) + (0 \times 0) - (1.22 \times 1.23)}{3} = -\frac{3}{3} = -1.0 .$$

So we may proceed. There are several important matters of principle at issue, as we see, long before a table of correlations is reached or its factor analysis begun. In the above case it is important to notice that information might be lost by transforming raw scores to standard ones. In the case of mental test data this is not the case, because the *mean* score gained by a sample of persons on a test is arbitrary, depending on the length of the test. Thus the standard

TABLE 4

	a	*b*	*c*
a	—	−1.0	−0.33
b		—	+0.33
c			—

TABLE 5

Statements	Person-Arrays (Raw Scores) A	B	C	Means
a	$1p$	$2q$	$1s$	?
b	$2p$	$1q$	$3s$	?
c	$3p$	$3q$	$2s$	?
Mean	$2p$	$2q$	$2s$	
Standard deviation	$0.82p$	$0.82q$	$0.82s$	

scores subsume all the important data, namely, the individual differences or dispersions of the scores about a mean value.

Similarly for system *2*, or Q-technique data. These might consist of single arrays for each of three persons A, B, and C, or of three arrays A, B, and C for a single person. The raw scores may be as given in Table 5 for a simple illustrative case. In this case the *statements* constitute a sample of some kind, and, of course, we would never in practice use only three statements. The person variables are not so defined or restricted. A forced distribution of raw scores is assumed for scores 1, 2, and 3, each of unit frequency. But A must be supposed to describe himself in terms of the three statements with respect to one unit p, whereas B has units q, and C units s. We can

never really specify what these units are, any more than we can do so for a reasoning test or a test of canceling letters. But the transitory postulate can be assumed to apply down the *columns* for such data, and differences in units can be removed by reducing the raw scores to pure numbers again, that is, to standard scores per *column*. The assumption is again made of common units down a column and acceptable transitoriness. There is no justification, however, for any standardization of these data along the *rows*. The data standardized

TABLE 6

Statements	Person-Arrays		
	A	B (Standard Scores)	C
a	−1.20	0.00	−1.20
b	0.00	−1.20	+1.20
c	+1.20	+1.20	0.00
Mean	0	0	0
Standard deviation	1.0	1.0	1.0

TABLE 7

	A	B	C
A	—	0.48	+0.48
B		—	−0.48
C			—

by columns are then as given in Table 6. We can now correlate by *columns*, two at a time, but not by *rows*, where the means are not zero and the standard deviations not 1.0 of *necessity* (as they are for each of the columns). The correlations for the person-arrays, for example, are given in Table 7.

Again several postulates are at issue, and the possibility exists that data may be thrown away by reducing the column means each to zero. As in R, we have to find ways to insure that no such important information is lost in the standardizing and that it adequately subsumes all the relevant data, dispensing only with the arbitrariness of all *units* as such.

It may be seen how erroneous it was, therefore, to suppose that only one matrix of data is ever at issue, fundamentally, which can be correlated either by rows or by columns as a matter of convenience. Each set of data involves its own postulates and assumptions. A matrix for R is one thing, that for Q quite another matter.

System *1*, then, leads to correlation between *tests*, and system *2* to correlations between *persons:* we should say, rather, between test-arrays in the one case and person-arrays in the other. R-methodology is always concerned with such correlations for test-arrays, and Q for those for person-arrays. Some confusion has arisen from the fact, however, that it is possible to proceed further with these matrices of

TABLE 8

(Same Data as for Table 3)

Persons	Tests: *a*	*b*	*c*	Mean	S.D.
	Standard Scores with Respect to Columns				
A	+1.22	−1.23	−1.06	−0.36	1.12
B	0.00	0.00	+1.32	+0.44	0.62
C	−1.22	+1.23	−0.26	−0.08	1.01
Mean	0	0	0		
Standard deviations	1.0	1.0	1.0		

data and to reach correlations between the *persons* for system *1*, and between the *statements* for system *2*. Because of the likelihood of this confusion, we adumbrated the two additional systems, *3* and *4*, which achieve these topsy-turvy correlations. The two systems do not involve new principles or lead to any new methodologies, as *1* and *2* do. They are not independent of *1* and *2*, respectively.

Systems *3* and *4* start with data which have been put together with respect to the various postulates of systems *1* and *2*, respectively, i.e., R and Q. The data are then *restandardized* so as to make the new correlations possible. We proceed as follows, for example, for the data of Table 3. The data are given in Table 8. For system *3* we now standardize these scores, which have already been standardized by *columns*, along the *rows*. The justification for doing so is that pure numbers are at issue—the transitory postulate is also remotely in-

volved. The data are then as in Table 9. The data can now be correlated by *rows*, i.e., with respect to the *persons* A, B, and C, since these are standard scores with respect to each person's array. The correlations in the present case are given in Table 10.

Similarly for system *4:* beginning with data for a Q-matrix, for which correlations between *persons* provide the first subsumption of data, it is possible to reach correlations between the *statements* by standardizing the already standardized scores of the Q-matrix.

TABLE 9

(System *3*)

Persons	Tests			Mean	S.D.
	a	*b*	*c*		
	Standard Scores along *Rows*				
A	+1.41	−0.78	−0.63	0	1.0
B	−0.71	−0.71	+1.42	0	1.0
C	−1.14	+1.31	−0.17	0	1.0

TABLE 10

	A	B	C
A	—	−0.45	−0.84
B		—	−0.12
C			—

Thus systems *3* and *4* gave the reminder that it was always possible to provide correlations between *persons* for an R-matrix, and between *statements* for a Q-matrix, *by resorting to this double standardization of the original data in each case.* It should be clear, however, that the correlations between the *tests* for system *1* and the *statements* for system *4*, or for the *person-arrays* for system *2* and the *person-arrays* for system *3*, are rooted in wholly different premises and concern very different matters. Those for systems *3* and *4* always involve doubly standardized data. Those for R- and Q-methodologies, instead, are for data standardized in one way only.

The *reciprocity principle* (41), by which Burt sought to prove that R and Q were directly reciprocal, applies to systems *1* and *3* and to

systems *2* and *4*. It can never apply of necessity to systems *1* and *2*, i.e., to R- and Q-matrices mutually. To think otherwise is merely to confuse the systems R and Q in the first place, and correlations for single and double standardizations in the second place.

SUMMARY OF QUANTITATIVE PRINCIPLES

From the outset (171), then, we regard R and Q as independent systems. The matter, we thought, was obvious. The following summary of postulates for R and Q places in perspective, perhaps, some of the matters we have had under consideration and lists others that deserve mention:

R-TECHNIQUE POSTULATES

i. The *populations* are groups of persons.
ii. Each *variate* has reference to an attribute or characteristic of all such persons.
iii. These variates do not interact—operations are according to the *rule of the single variable.*
iv. The transitory postulate (namely, if $x > y$, $y > z$, then $x > z$) proceeds in terms of *individual differences.*
v. Scores are reduced to *standard scores* with respect to each variate, for the sample of persons concerned.
vi. These scores are approximately normally distributed with respect to the sample of persons.
vii. All the important information for each array is contained in its variation (no information is lost in throwing away the variate *means*).
viii. The concern is with interdependency analysis.

Q-TECHNIQUE POSTULATES

i. The *populations* are groups of statements or the like.
ii. Each *variate* has reference to an operation of a single person upon all the statements in one interactional setting.
iii. The *variates* may interact in the one interactional setting.
iv. The *transitory postulate* has reference to *intra-individual differences* (such as "significance").
v. Scores are reduced to *standard scores* with respect to each person-array.
vi. Scores are approximately *normally distributed* with respect to the person-array.
vii. All the important information for each array is contained in its *variation* (no information is lost in throwing away the variate means).
viii. The statements of a sample may interact.
ix. The concern is with *dependency* analysis.

Sufficient has been said, in the foregoing pages, about postulates iv, v, vii, and viii in each case. What is represented by *standard*

scores, the *transitory postulate*, and the *units* implied cannot be overlooked with impunity, as we have seen. Thomson (181) was pessimistic about Q-technique almost entirely because he conceived of the transitory postulate in relation to individual differences only. Nor did it occur to Burt (41) that this same postulate might apply in its own right to *intra*-individual "significances."

The references to *populations* will be considered in the next chapter. Some mention has been made earlier of the status of variables in R and Q—those in R being for interdependencies only, whereas in Q the concern is with dependent variables. Questions about the operation of the "rule of the single variable" will be raised later; meanwhile, it is obvious that in R each test operates according to this rule—the subjects can respond to only one test or question at a time, and it is assumed that what they do on one test will not, ordinarily, influence them on what they do with another test. Everything possible is done, in mental test construction, to achieve conditions which will be such as mediate this rule. In Q, on the other hand, all the statements of a sample have to be compared with one another, and judgments must be made about each statement in the context of all the others and the conditions of instruction. We shall see, again, that the *structure* we give to samples makes clear that the rule of the single variable does not apply to Q, which, instead, makes use of the Fisherian procedures to deal with main effects and the interactions in their natural contexts.

THE FORCED-CHOICE METHOD

Postulates i to ix for Q-technique are all at issue in the forced-choice method which is so characteristic a feature of Q-technique. The method will be regarded as highly arbitrary if one is bound to R-methodological principles. It has an experimental basis, however, founded upon Fisher's methodology, as we shall see later. We argue that if all were error for a Q-sort, a normal distribution of scores would be expected for it. But we anticipate significant factors or effects, and therefore a normal curve is not likely. For certain reasons, however, it should be symmetrical. Therefore, we follow the practice of using a much flattened symmetrical distribution of scores for all Q-sorts.

Operation ii for Q is intrinsically quite simple. A sample of N state-

ments, usually 70–150 in size, is well shuffled, and the operator sorts these into ten or more classes, on a quasi-normal frequency basis, as in Table 11. We make no claim to be the originator of this very simple operation, for something of the kind has been in existence, probably, ever since there was a schoolmaster to mark essays. But we give the operation very wide scope and make it serve to provide the quantitative basis for almost any subjective behavior.

In practice, attention is first given to all N items by the operator, who gains a "general impression" about them as a whole and probably reaches something akin to "choice equilibrium" in relation to them (analogous with the affective equilibrium first described by Beebe-Center, 18, for samples of *odors*), before following the instructions which require him to place items highly "significant" (positively) at the high-score end and those negatively "significant" at

TABLE 11

	Most Significant											Least Significant		
Score	12	11	10	9	8	7	6	5	4	3	2	1	0	
Frequency	2	4	6	8	10	12	14	12	10	8	6	4	2	($n=98$)

the zero-score end, with items judged to be doubtful, neutral, or the like at the center of the distribution. A minimum of 10 classes is advisable, for a flattened, platykurtic distribution and not a normal one. It is important that the operation itself should be a reasonable one, such as a person can perform without feeling that it gravely distorts what he wishes to do.

The operation is clearly more complex than one, for example, that calls upon the operator merely to make a two-point choice (YES or NO) about each item in turn, as in the *Multiphasic* or similar schedules. All the statements have to be compared with one another, however loosely. Fine discriminations are not involved for every item, however, as would be the case if all N items were to be ranked: the largest proportion of the items are placed in the middle classes; and, although discrimination is most difficult at this point, its importance is reduced by the fact that in product-moment correlation the end-classes gain most weight. Everyone has to follow the same distribution, thus obviating the idiosyncrasies which are an all-too-fa-

miliar feature of the *absolute* method in psychophysics (187) when there is complete freedom of scores.

It is not difficult to suppose that the forced-choice method is merely an example of the *constant methods* in psychophysics, more especially of the method of *single stimuli* and, generally, of the *law of comparative judgment* (185). Actually, the differences between Q-sorts and those of the psychophysical methods are fundamental. In the psychophysical methods the aim is to *reduce* experimental error (chance) to a minimum. Weights were lifted a hundred times to achieve the necessary degree of accuracy of correct or incorrect judgments; experimental or chance errors were eliminated. The methods for computing threshold values, such as those of *linear interpolation*, or Spearman's method of the *arithmetic mean*, or Kelley's *least-squares* procedure, are imposed upon the observed proportions of judgments which were supposed to have reached the necessary high degree of accuracy beforehand. In Q-technique, as in Fisherian design, the aim is not to eliminate experimental error but to make an explicit estimate of it *where it is expected.* These principles, however, cannot be made any clearer until we deal with the contributions of Fisherian methodology to Q, a matter with which we shall be concerned in the next chapter.

CHAPTER IV

SAMPLES AND THEIR STRUCTURE

UNIVERSES AND THEIR SAMPLES

WE ARE to continue the discussion of the differentiation between the R- and the Q-methodology, now with reference to the *samples* they employ. The customary idea about a sample is that it is drawn, in some random manner, from a parent-universe or "population." Large-sample conditions are at issue in R. But a distinction must be drawn between *populations* and the *statistical universes* which may be derived from them. A sample of 200 children is a population sample, but their scores on a test are a statistical distribution, and for any one population there may be innumerable statistical universes. R-factorists and psychologists are usually not much concerned about formal consideration of populations; but pollsters, market researchers, and social scientists have devised *area*, *stratified*, *biased*, *controlled*, and similar devices in order to draw upon population samples for which each person will have an equal chance of being chosen or to make allowances for departures from *representative* sampling. The careful definition of population conditions by Roberts (127), the Scottish Council for Research in Education (124), and others is testimony to some attention to populations in psychology, but factorists are principally involved in the study of statistical universes. Each of the latter is for a single attribute (such as height, scores in test X, assessment for a particular *trait*, or the like); but they may be gathered from person samples which are characterized more by biases of various kinds than by any formal representativeness of a parent-population. Thurstone (188), for example, expresses little concern about the latter, and a recent survey of R-factor studies by Vernon (196) makes little mention of populations or their sampling conditions. Attention is centered, instead, upon approximations to normal distributions of measurements; and the concept of "random sampling" appears to have reference to the supposition that, in the measurements for any

attribute, no one measurement is contingent upon any other for any person. It should mean, classically, that neither in the selection of the population sample nor in the measurement of any attribute for it is any one person or any measurement of him contingent upon another. Theoretically, the correction of reliability coefficients for homogeneity of samples (114) in R-technique (as in Q) offers testable operations with respect to the statistical universes at issue.

There are at least two reasons for the R-factorist's lack of concern with populations as such. First, there is a presumption that factors tend to be invariant and that what is discovered for an odd assortment of urban children will be yielded as certainly for a set of children drawn from even a single city school. Second, the factorist is likely to define his population samples in terms of the statistical distributions they yield, rather than the reverse. The two together conspire to make the *ab initio* postulation of population universes of minor interest in factor analysis. Tests are constructed for which there is a reasonable expectancy or presumption that a "random" sample of persons, for which the test is suitable, will provide a quasi-normal frequency distribution of scores. If the expected distribution does not occur in fact, either the test is altered, or it is supposed that the sample was inadequate and it is replaced by a better one. In this way the statistical distributions and test construction govern the population universe, which becomes an abstraction and not a concrete operational matter. "Random" sampling is thus something of a fiction in R-technique. R-factorists, however, appear to get along quite well in spite of these departures from classical large-sample doctrine.

Populations in Q-technique have been composed of *statements* (174), *art objects* (172), *descriptions of behavior* (161), *personality traits* (167), and the like; and it seemed almost impossible to define population universes for them or to provide an acceptable basis for sampling any that might be so defined. It was easy to point to gross heterogeneities and to obvious contingencies (12) which must break the most elementary rules of random sampling. Even so, some progress was made.

One of our earliest universes consisted of all the *statements* made by Jung (164) about his psychological types: some two thousand statements were collected, and the earliest notion was that samples

could be drawn from this parent-universe at random. Similarly, several hundred picture postcards of famous paintings were collected and used as a parent-population for subsequent sampling (162). In still another case several hundred photographs of *vases* were made to constitute a parent-universe: it contained a heterogeneous assortment for many different *eras* of vase-making, centuries apart, for vases which were originally meant to serve many different purposes. It was no doubt difficult for statisticians to regard such agglomerations as "populations," however large their size, even if the prejudice that populations should be for *persons* only could be offset.

But two rather different sources of confusion exist in relation to Q-populations. One concerns the kind of multivariate analysis which is to be undertaken for them, and the other the necessity of distinguishing, precisely as for R-technique, between the *populations* as such and the *statistical universes* which are derived for them. The first is related to the matters of interdependency and dependency analysis, which we have mentioned earlier. With respect to the second, it is clear that the definition of a population of paintings, vases, statements, or the like is not a statistical matter (except that large numbers are involved): no scores or quantities are anywhere involved, and no frequency distributions. But if we take a set of paintings or the like and ask a person X to score them on a 10-point scale according to which he *likes* most, frequency distributions and statistical universes are, of course, at once at issue. The psychologist has long been familiar with such distributions in connection with applications of the psychophysical methods; the scoring of paintings is an application of the method of single stimuli (206). Thus, although the populations might well be ill-assorted, heterogeneous, and unrepresentative, the statistical universes derived from them could be orderly and amenable to precise formulations.

This, however, does not solve difficulties about the definition of the populations in the first place. One would wonder how far any set of pictures could be a *representative* sample and what, if anything, it could represent. In principle, no doubt, all vases in existence could be listed, and the procedures followed in drawing representative samples of persons could be employed for vases. We doubt whether such matters were contemplated by those who were dubious about the status of the populations we are considering at the moment. Nor

is the matter of heterogeneity difficult to dispose of. One of our early studies (162) made use of a sample of 60 colored photographs of vases. This did not consist of *any* photographs of vases; all were colored and were similarly reproduced. Several were taken from each of the great eras of vase-making, from Ancient China, Greece, Egypt, and Europe. No two were obviously alike. What was achieved was a certain homogeneity with respect to *vases*—anyone, looking at the 60, would tend to regard them as of one class, alike in a very substantial fashion; but also a definite heterogeneity was achieved, such that would lead one to say that, even so, all the vases are different ones. The statistician is likely to think of populations as necessitating such conditions of homogeneity in kind and of heterogeneity and noncontingency among the items of a kind.

These conditions, however, do not define populations so much as they lend themselves to satisfactory operations leading to *statistical universes*. Thus, if a person X is asked to appraise a set of 60 vases (for which ones he *likes* best), what is essential, from the statistical point of view, is that his judgments should be independent and separately given for each vase. Each item of the sample has to be appraised as a distinct and separate act or operation on X's part. This would not be the case if judgments were made about 10 vases at a time, as might happen if 10 of them were obviously alike in a very distinctive manner (such as all 10 Greek, or all 10 Chelsea) and were given the same score on that account. Still less would this be the case if 10 oil paintings were introduced into the sample of vases. The scores in such events would not have been given independently. Thus care about homogeneity of class and heterogeneity within it is directed toward operational matters of this kind, and therefore toward the necessity in statistical distributions for the items to be independent and not contingent in any obvious manner upon one another.

This does not necessitate, however, that the 60 items must be judged *without reference to one another*, a very different matter. On the contrary, X may look at all 60 before beginning the appraisal of each and might make innumerable implicit comparisons in so doing. This is always at issue in Q-technique. *The concern is with a setting in which all the items may interact in relation to X.* We may suppose, however, that a certain equilibrium will be reached, relative to which X can make his judgments. Beebe-Center (18) has already drawn

attention to conditions of this kind and has stated the interactional effects in terms of a "law of equilibrium." Even so, the acid test from the statistical standpoint is whether the judgments made by X are such that each is an independent appraisal, none operationally contingent upon any other. It was this, perhaps, rather than any other reason, that caused misgivings among the statisticians (Babington-Smith, 12), who saw little reason to accept lack of contingencies as a postulate. In large-sampling theory there is no test for the matter at issue, and one could merely seek to achieve conditions which would favor it, such as we have been discussing. The situation is very different in Fisher's experimental methodology (68).

These considerations apply to all Q-technique samples of an apparently random kind. But there are still other difficulties to be met. Universes in Q-technique are often of restricted size: there are probably not thousands of modes of behavior of a caged rat, and not even Picasso can provide an infinite number of paintings for our study. Corrections (114) could be made, of course, for restrictions in the size of parent-populations, provided that there can be some confidence about the latter in the first place.

There has been, therefore, no really adequate rationale for putting together populations in Q-technique along large-sample lines, except, as in R-technique, to define populations abstractly, in terms of the *statistical universes* one could attain and not in terms of conditions of the populations in the first place. The necessity for some kind of rationale for the populations in Q-technique, however, was a pressing problem for which there were no precedents other than in large-sample doctrine. The use of small-sample doctrine and structured sampling, to which we are now to address ourselves, has altered the situation entirely.

THE STRUCTURE OF SAMPLES

Structuring a sample consists of composing it artificially, instead of selecting it at random from a parent-universe. This will seem somewhat radical, until it is realized that a place is kept for random operations in the way the samples are used. The structure is achieved by applying Fisher's (68) methods of experimental design to samples.

An example for a structured sample in R-methodology will make

the method clear. The concern is with the definition of population samples. A sample for use in R could be composed for the balanced block design given in Table 1.

The concern, we shall suppose, is with white American males, and the specifiable independencies or "effects" are age, socio-economic class, educational status, and habitat. That is, we propose to be concerned with adults aged fifteen to thirty, men of each social status but with greater emphasis on A and B, men who have been or are at a university or high school and who live in cities or in rural communities. Normally all such "effects" are only loosely defined or specified in studies. Here, however, all of them are taken into account when drawing a sample, in such a way as to have them all

TABLE 1

Independencies	Levels	No.	D.F.
A, Age.	(*a*) 15–20 years (*b*) 20–30 years	2	1
B, Socio-economic status.	(*c*) A (*d*) B (*e*) CDE	3	2
C, Educational status. . .	(*f*) University (*g*) High school	2	1
D, Ecological status.	(*h*) Rural (*i*) City	2	1

in balanced design, that is, so that each effect for any level has all the others and their levels represented equally.

There are 24 combinations of the independencies in Table 1, one level at a time for each ($2 \times 3 \times 2 \times 2 = 24$), namely:

a a a a	*a a a a*	*a a a a*	*b b b b*	*b b b b*	*b b b b*
c c c c	*d d d d*	*e e e e*	*c c c c*	*d d d d*	*e e e e*
f f g g	*f f g g*	*f f g g*	*f f g g*	*f f g g*	*f f g g*
h i h i	*h i h i*	*h i h i*	*h i h i*	*h i h i*	*h i h i*

We can choose 24 men, therefore, to cover these 24 possibilities once each. The combination *a c f h*, for example, will be a man in the fifteen to twenty age range, of socio-economic level A, who is attending or has attended a university, and who lives habitually in a rural community. Men can be found to cover each of the other combinations in turn. But the design can be replicated any required number of times, so that the size of a "sample" can be as large as we please, in multiples, in this case, of 24 men. Clearly, any number of "levels" may be specified, or "effects," although large numbers of these soon make matters impracticable and reach into astronomical figures for

the size of the samples. The design itself is the same as that first employed in psychology by Crutchfield (56); in his case, however, the concern was not to define a sample of rats but to define an experimental situation into which to place an unstructured set of rats *randomly*, a very different matter.

The concern, clearly, is to select persons according to plan, in such a way as to control specifiable or possible influences, effects, or independent variables, doing so in a formal manner, instead of leaving these lying about in an undetermined and unspecified way in the random samples of persons that would otherwise have been chosen by random sampling procedures. At face value, no one could distinguish between a structured sample of persons and one randomly selected from the same parent-population. Each sample can be repeated at will, with some guaranty that any one will be like any other, as far as these "effects" are concerned. When samples of persons are required of size 100–200 or so, structured sampling suggests itself. The design specifies what the parent-population or universe will be, theoretically; samples are then put together as required, in such a way that, in principle, each is comparable with any other. Their size can be as large as we please, in multiples of the basic number of combinations of effects and levels. Clearly, *statistical universes* are in no way at issue.

In relation to our earlier remarks about population universes in R-technique, the use of structured samples may seem to be of little account, since populations are not of much concern to the technique. Yet the samples which are used certainly involve independencies of the kind that can be made explicit in a formal design, and this may offer some advantages, since everyone would know what is involved, at least, if a specified balanced block sample is employed as the basis for samples. As things are at present, no one is quite sure what constitutes a sample in factor analysis of the R-technique form. However, our more immediate purpose in describing the structuring of R-samples has to do with an important difference between R- and Q-techniques in this respect. When *statements* and the like constitute the "populations," as in Q-technique, structured samples are of surprising theoretical and practical interest, surpassing by far their implications in R-technique. It is to these matters that we now turn.

Meanwhile, populations of persons are of such importance to

social scientists that the use of balanced designs may well have some practical use among pollsters and market researchers. It is an interesting question to ask, for example, whether any *necessity* exists for representative sampling of such a kind that the samples are replicas in miniature of a larger parent-population and whether artificial samples, for balanced block designs, could serve many of the scientific and practical purposes for which it has been usual, up to now, to use large-scale random sampling.

STRUCTURED SAMPLES IN Q

The method of structuring a sample in Q-technique was first employed in one of our early papers (156), and reference was made to it again more recently (166). It provides a *rationale* for the construction of population samples ad lib in Q-technique.

TABLE 2

Independencies	Levels	No.	D.F.
X, "Attitudes"	(*a*) Introversion (*b*) Extroversion	2	1
Y, "Mechanism"	(*c*) Conscious (*d*) Unconscious	2	1
Z, "Functions"	(*e*) Thinking (*f*) Feeling (*g*) Sensation (*h*) Intuition	4	3

One of the first samples of the kind concerned Jung's type psychology. This, in its simplest form, specified three main issues: (X) the "attitudes" of *introversion-extroversion;* (Y) the "mechanisms," *conscious-unconscious;* and (Z) the "functions," namely, *thinking, feeling, sensation,* and *intuition.* We proposed to regard these as independent variables, or *independencies,* and to construct a balanced block design to cover them (Table 2). This leads to 16 ($2 \times 2 \times 4$) combinations of the independencies, one level at a time, namely:

a a a a	*a a a a*	*b b b b*	*b b b b*
c c c c	*d d d d*	*c c c c*	*d d d d*
e f g h	*e f g h*	*e f g h*	*e f g h*

In order to clothe it with *statements,* as Table 1 was covered by *persons,* we merely take assertions by Jung (87) which comport with these combinations, one statement to each combination. Thus Jung's statement "ready to sink a battleship or to amputate a leg"

would fit into *b c f;* "quietly sensual" into *a d g*. Sixteen statements of this kind can be found to cover the 16 possibilities: a typical set, for example, could be as follows:

a c e	1	........Seems reposed, inconspicuous.
a c f	2	........*Theories* are important to him, *facts* not.
a c g	3	........Has little understanding of himself.
a c h	4	........Brusque.
a d e	5	........Vain.
a d f	6	........Has a vague dread of the opposite sex.
a d g	7	........Lacks self-judgment.
a d h	8	........Always has the right answer.
b c e	9	........What he can't *feel* he can't *think*.
b c f	10	........Has lapses into shortsightedness.
b c g	11	........Charming capacity for enjoyment.
b c h	12	........Establishes the right social contacts.
b d e	13	........He breeds half-resolves.
b d f	14	........Resentful, dogmatic.
b d g	15	........Superiority feelings.
b d h	16	........Oversubtle in reason.

But we can replicate and find or devise, say, 5 statements for each of the above 16 combinations, making a structured sample of $5 \times 16 = 80$ statements in all. If required, the replications can be drawn at random from pools of the statements of each "cell" or class. Clearly, the size of the sample depends upon the number of independencies, "levels," and replications specified. Theoretically, any number of samples can be composed for the design of Table 2, because any desired number of replications and any one sample are, in principle, as representative as any other. The design, as in R-technique, defines a population universe in principle, from which comparable samples can be composed at will.

It will be a matter of some interest to see a structured sample, and Appendix I, therefore, gives a sample ($n = 80$) for Table 2, taken from Jung's "type psychology."

If the situation is compared with that for a *random* sample drawn from a presumed parent-population or defined universe in Q-technique, such as for a sample of 100 statements drawn at random from the 2,000 referred to earlier, the practical implications of structuring are at once apparent. Jung provided many more statements descriptive of introversion than of extroversion, and more *thinking* and *feeling* functions than of the others. Any randomly chosen sample

would therefore be biased by these predilections of Jung, and a balanced block design merely sorts the issues out in an explicit manner, so that all such possibilities will be taken care of equally and, also, so that multivariate analysis will be possible if the statements can be quantified. In short, the preconditions for statistical analysis and sound inferences are implicit in these structured samples.

QUANTITATIVE PRINCIPLES

There is much to consider, however, about the implications of structure and about the twist we are giving to the status of samples.

TABLE 3

		D.F.
ΣA (age)		1
ΣB (socio-economic status)		2
ΣC (educational status)		1
ΣD (ecological status)		1
Double interactions	ΣAB	2
	ΣAC	1
	ΣAD	1
	ΣBC	2
	ΣBD	2
	ΣCD	1
Triple interactions	ΣABC	2
	ΣABD	2
	ΣACD	1
	ΣBCD	2
Quadruple interactions	$\Sigma ABCD$	2
Total		23

It should be clear that the concern so far is with the definition of *population samples*, whether in R or Q, and not with any *statistical universes*. The latter are provided, of course, by operations upon the samples, structured or not. Thus, if a particular test X is applied to 24 men for a balanced sample for Table 1, the scores gained could be analyzed along familiar lines, for the division of the *variance* and its 23 degrees of freedom shown in Table 3.

The single effects could be examined for significance in terms of the interaction "expectancies" for 18 degrees of freedom in the usual F-test manner and small-sample theory. Or, if 10 replications are involved, so that the sample is of size $n = 240$, there will be 216 degrees of freedom against which to test all the effects. The

replication variances would be tested for homogeneity (33) in the usual way. If many tests were correlated and factored for such a sample of men, each factor-array could be so analyzed by small-sample rules of procedure. Whether it is of any immediate interest for anyone to make such studies is neither here nor there at this point; clearly, "objective" matters would be at issue for a sample structured as in Table 1, the concern being, probably, to *control* the independencies so specified. No doubt certain psychological theories can be formulated for such designs, as well, a matter we shall consider in due course; but it is obvious that the theories will be very different from those represented in Q-populations.

TABLE 4

	High Significance									Low Significance		
Score	10	9	8	7	6	5	4	3	2	1	0	
Frequency	2	4	6	9	12	14	12	9	6	4	2	($n=80$)

TABLE 5

	D.F.
ΣX (attitudes)	1
ΣY (mechanisms)	1
ΣZ (functions)	3
ΣXY	1
ΣXZ	3
ΣYZ	3
ΣXYZ	3
Σ (replication) (16×4)	64
Total	79

In Q-technique the same small-sample doctrine is applicable for structured samples such as Table 2 illustrates. It is the purpose of a Q-sort in Q-technique to provide the quantitative data for its samples. Thus, given a Jungian sample of $n = 80$ statements for Table 2, in balanced design, i.e., for 5 replications, we can call upon *any* person X to offer a self-description in terms of the sample, that is, to score them on a forced frequency-distribution basis for their "significance" in his personality (from his internal frame of reference), e.g., for the frequency distribution shown in Table 4. The variance for the scores would be analyzed for the divisions given in Table 5. The replication variance would be tested for homogeneity

(33), and the other effects studied for significance by F-tests. Normally, as in agronomy and industrial applications, the higher-order interactions may be employed along with replication to boost the "error expectancy" estimation. *In general, if the data are proved homogeneous at the point of replications, we may suppose that the principle of randomization (68) has operated adequately during the person's self-description and that contingencies are not involved where they were not expected.*

It will be clear that variance analysis can be undertaken for every single Q-sort, prior to any factor analysis of the correlations for several Q-sorts for the same structured sample. Or the variance analysis can be applied to factor-arrays, i.e., to the factors themselves that issue from any factor analysis of correlated data with respect to the sample.

GENERAL THEORY OF STRUCTURE IN SAMPLES

Formally, the procedure for structuring a "sample" is as follows: If there can be defined certain "effects" or independencies $A, B, C, \ldots$, with "levels" $a, b, c, \ldots$, respectively, then, without replication but with a balanced block design, there are $a \times b \times c \times \ldots$ combinations, one level at a time for each effect. If these are replicated m times, there will be $m \times abc \ldots$ such combinations. We define a *structured* sample in this way, to consist, generally, of a set of $mabc \ldots$ cases in balanced design. But it is possible to make use of other designs as well, such as Latin Squares and the like.

These designs are remarkably versatile, especially in Q. It is possible to represent almost any theory of personality or behavior in this way or to deal with lesser hypotheses or conclusions in the same manner. Theories in psychoanalysis, clinical, social, self-theory, personality, and other branches of psychology can now be expressed in this formal way. *Conclusions* to researches can be formulated as factorial designs and put to proof. Ecological universes (35) of behavior can be represented, or whole segments of a person's life-history. It should be added, too, that *unstructured*[1] samples, such as the early ones were in Q-technique, are to be regarded as inherent-

1. In point of fact, they are never completely unstructured but, at least, are balanced about two "levels." Thus if a collection of statements is taken from patients at a clinic about the condition of their health, a simply balanced sample would be one containing as many statements about ill-health as about health.

ly capable of structuring and, therefore, that independencies are at issue even if they cannot be specified explicitly at the outset.[2]

It has to be asked, however, under what conditions a structured sample is acceptable to scientific regard. We note, first, that structured samples as such cannot be distinguished from the customary randomly selected ones, if one does not have prior knowledge about their structure. They look like random ones if the structure is ignored. They differ from random samples, however, in two important respects—first, where *error* is to be expected, it is specified in a structured sample and not in an unstructured one; second, independencies are represented formally in the one and not in the other.

It was mentioned earlier that a Q-sort is not to be compared with any of the classical psychophysical methods, nor is Thurstone's famous *law of comparative judgment* at issue in Q. In the psychophysical methods random *errors* were meant to be eliminated experimentally. In Fisher's methodology the replication of experimental conditions provides an estimate of error variance, which is then used as the basis for judging the significance of all systematic effects, that is, by way of *small-sample theory* and the familiar F-test procedures. Neither in the law of comparative judgment nor in factor analysis are error estimates ever employed in this way. Sampling errors in correlational and factor studies, for example, are theoretical matters —the standard error of a correlation coefficient is given by the theoretical expression: $\{1 - r^2\}/\sqrt{n-1}$. The error variance in many factor studies, likewise, may be larger than that considered as *communality*—the factor loadings derived from a factor analysis are often all smaller than the error variance for the same table of correlations. Fisher's methods are grounded, instead, in the actual operations at issue, and estimates of error are thus essentially empirical matters. If an experiment is successful in factorial design, the "expectancy" (or estimate of the "population" variance) for error conditions has to be smaller than that for any of the experimental effects of their interactions. Fisher's methods have this sort of good

2. It would be a mistake to suppose that we are to recommend that all samples in Q-studies should be structured ones; on the contrary, some of the best work possible can proceed without them. All that is at issue here is the formal explication of Q-populations in general, whether they can be or need to be structured or not.

sense at their roots. They clearly represent a major advance in experimental methodology, paralleling, in their reference to operations, many recent developments in the logic of scientific method. Again, in classical psychophysics and basic also to the law of comparative judgment, experiments were conducted according to the "rule of the single variable" (68). It was regarded as essential to vary experimental conditions one at a time, all else being the same. Fisher's procedures are upon a much wider basis, as is well known, covering multivariate conditions and interactions as well as single effects.

Q-technique takes its stand with Fisher's methodology. In the case of a structured sample, analysis can be pursued for the distribution of scores provided by a single Q-sorting; the replication variances provide the error "expectancy," and these can be tested for *homogeneity* (33). *This informs us at once whether experimental conditions have really been grasped.* We regard this test as crucial and basic to all else in Q-methodology. We are prepared, for example, to judge whether factor loadings of a factor study are statistically significant in relation to such error estimates in a variance design, rather than to have to rely upon standard errors of correlation coefficients, as derived from large-sampling doctrine. So much, then, for the first of the two ways in which structured and random samples differ.

As for the second, critics will ask at once upon what grounds an investigator allots statements or the like, in Q, to the structure of a balanced block design. Does he just proclaim that the statements, so allotted, are the embodiment of the theory at issue, or does he seek by objective means to place statements correctly in their places in the design?

The answer, very briefly, is that the structure represents a theory which we do not seek to prove for its general implications; therefore, there can be no "correct" place for any statement or item of a sample. The theory has to be proved indirectly, by way of singular propositions about it. In Q-method the concern is with these samples, but also with *variate designs.* The latter represent singular propositions that are being tested, and this testing can be pursued without regard to the structure of the sample, namely, by way of factor analysis. But when we seek to *explain* the results of a factor analysis, we turn to the variance analysis of the sample. Thus

propositions are tested by factor analysis, and *explanations* of the factors are tested by variance analysis of structured samples. That is, we keep theory at hand, so to speak, in the sample; but we wish to *use* it, not to prove it directly. Its purpose is to explain factors that we arrive at by way of Q-technique operations.

This is in keeping with our view of theory as a growing-point for testable propositions and with the fundamental principle that science is concerned with the testing of singular propositions. Under no circumstances do we look to any operations to *prove* that the apportioning of statements into the cells of a factorial design are "correct" with respect to any general implications or propositions. To attempt anything of the kind would merely bring us back to R-methodology and all its mistakes. Thus our essential interest is always in facts that we hope to reach in relation to our samples. One sample may be used in numerous studies, all different, and each involving perhaps several propositions that are under empirical test. If these are verified, it is then of some interest to ask about their more general *explanation*. This is what has been embodied in the structure. The proof of a theory then becomes a matter of how many and varied are the different singular propositional sets it assists us to devise and to understand. What is general, therefore, is one's theory or explanation, and never any proposition or the operations that put it to empirical test.

SOME PRACTICAL CONSIDERATIONS

Thus statements are placed in the factorial design on theoretical grounds. Certain precautions are taken, of course. When the R-factorist puts together a sample of children, he is careful to exclude apparent anomalies, such as the blind or the deaf-mutes. Similarly in Q, *any* sample of statements put together theoretically is, in principle, as acceptable as any other for the same design, but care is taken about such matters as conciseness, clarity, representativeness, and the like. It is essential, as was mentioned in the case of the vases study, to achieve a certain homogeneity in the sample, so that no item in it is picked out for special regard on any extraneous or incidental grounds. This may happen, for example, if a statement is too difficult for anyone to understand. In the case of the example for Jung's theory of introversion-extroversion, the universe of state-

ments from which structured samples were drawn is contained in Jung's own writings: the statements which are descriptive of an intuitive extrovert (unconscious mechanism) are those employed by Jung himself. After all, it is his theory we wish to study. Again, however, certain precautions can be taken. In Jung's case, for example, his language presents difficulties—some of it is highly *technical*, some *metaphorical*, and some of a *common-sense* variety. For some purposes only the latter will be convenient. But a structured sample can also be composed in which *language* is represented as a possible effect, with these three different language forms as "levels." The balanced design for Jung's case is then modified as shown in

TABLE 6

Independencies	Levels	No.	D.F.
A, Attitude. . .	(*a*) Extrovert (*b*) Introvert	2	1
B, Mechanism . . .	(*c*) Conscious (*d*) Unconscious	2	1
C, Function.	(*e*) Thinking (*f*) Feeling (*g*) Intuition (*h*) Sensation	4	3
D, Language.	(*i*) Technical (*j*) Metaphorical (*k*) Common-sense	3	2

(Number of Combinations $2 \times 2 \times 4 \times 3 = 48$)

Table 6. It is then possible to determine how far language mediates as an effect in one's Q-sorts.

The structuring of a sample provides for its representativeness. For example: Jung uses far more language in describing introverts than extroverts; and merely to select statements at random from his treatise would weight the sample unduly with Jung's own predilection. The structured sample obviates idiosyncrasies of this kind. The investigator is also saved from any necessity to be exhaustive: any one sample is, in principle, as acceptable as any other, and it is always possible to put together additional samples for any given design, almost indefinitely. There is a certain art in compiling neat statements or the like, suitable to the needs of a particular study. Statements for use with parents of mentally defective children about their children have clearly to be different from those for use by university students. But it is a mistake to regard a sample as a standardized set or *test* of statements, any more than one can hope to regard a particular set of children as a standard sample for R-tech-

nique purposes. Each of several investigators could have his own sample for a particular area of study, even if all were engaged upon the same problems.

One or two other practical matters about Q-samples deserve brief mention. It is often necessary to collect a universe of statements[3] for one's investigations. To help in this, Brunswik's (35) concept of an "ecological" universe is helpful. Thus for one study we kept an account of all the incidental references made by members of our family about things *American*, for the first six months of its stay in this country. The statements were gathered in naturally occurring situations, at meals, play, and during visits to town and the like. After many statements, of the kind "I *like* Americans, you know," had been collected, they were edited and were then ready for fitting into factorial designs. Similarly, if one counsels a subject X, an account can be kept of all self-referent statements made during interviews—these may cover a wide behavior segment of X's life, concerning interactions at home, college, with friends, at work, and so forth. These would be statements made *consciously*. But the same behavioral segments may be covered by suitable projection (TAT) tests, and statements gathered that X makes about *others*, with apparent projection. Statements may be collected from a subject about himself in relation to his college milieu—his aspirations, his friends and members of the opposite sex, and the like, as expressed in interview sessions, in a daily journal kept by the subject, and the like.

In all such cases the result is a rough-and-ready universe of statements. Where factorial designs are at hand, the structured samples are taken from this universe. A small committee of judges may be used, if necessary, to compose samples. When no factorial design is available, other considerations govern the selection of a sample from such universes, chief of which, perhaps, are the following: (i) The sample should be *balanced* with respect to at least one effect. (ii) It should be homogeneous, as judged by the ease with which the transitory postulate can apply for any operator. The sample is thus never selected purely randomly from the universe. It is usually a simple matter to achieve condition i: thus, for every

3. Throughout these chapters the term "statement" is used to cover any item of a Q-sample, whether this is a verbal statement, a drawing, an art object, or a trait name.

statement with a *positive* assertion or meaning, there can always be chosen another with a *negative*—for "good," the word "bad" is an example. But mere negations are not recommended, e.g., "honest" and "dishonest" would never appear in the same sample because of the possibility that a contingency may be involved in operations by a person upon the sample containing the antonyms. This would happen if X put the antonyms together and decided that they should have $+x$ and $-x$ marks, respectively. A set of statements about *ill-health* can be balanced with another about *good health*, or statements known to be *true* about X can be balanced with "blinds," that is, statements known not to be true in X's case. Balanced samples, although not fully structured, satisfactorily deal with the *distensive zero*,[4] or with the postulate of Q-technique which says that all the essential information should be contained in the Q-array's variation and none of importance in the *means* of such arrays. Consideration ii above can be dealt with only in empirical terms, the acid test being whether the operator finds it possible to provide the necessary forced distribution without a sense of distorting matters at issue.

ADDITIONAL METHODOLOGICAL CONSIDERATIONS

Theories of the kind we represent in Q-samples, such as Jung's, Freud's, Rogers', or Murray's, or theories about the "self" or about "form" in aesthetics are scarcely to be thought of as neat, confined, hypothetico-deductive systems, comparable to Euclidean geometry or Newton's *Principia*. Rather, they are such that only multivariate forms of analysis can examine, and the samples merely hold some of the possibilities that a theory entails.

The whole of a theory, as a rational matter, can rarely be represented in a structured sample, nor is it necessary that it should be. If our interest is in the psychoanalytical doctrine, for example, we could make a beginning with a study of "character" development: two effects might be specified, (*A*) the developmental ones, with oral, anal, and genital levels, and (*B*) the resulting "characters," such as hysterical, anxiety, compulsive, and normal. The balanced block design is at once apparent (Table 7). This does not cover all the "characters" nor does it involve the explicit aspects of thwarting and gratification or of repression or the reverse. But any such addi-

4. See p. 196.

tional effects and their levels, or additional levels for B, can be added at will. Meanwhile, a beginning can usually be made with a simple design—the statements for it, in any case, will usually be those that would be further subdivided and apportioned among any additional effects or levels that one might introduce into the design. In the same way, we would never seek to represent in a sample all the possibilities connected with Cézanne's use of shape in a theory of aesthetics, but we can represent his theory in a context of simple shapes, compose the necessary sample of aesthetic patterns, and study many interesting consequences as testable propositions, all in a manner hitherto impossible of achievement. Similarly, one would scarcely hope to represent every facet of Professor Rogers' client-centered theory in a single structured sample: instead, it would be

TABLE 7

Independencies	Levels	No.	D.F.
A, Developmental....	(*a*) Anal (*b*) Oral (*c*) Genital	3	2
B, Characters........	(*d*) Hysterical (*e*) Anxiety (*f*) Compulsive (*g*) Normal	4	3

(Number of Combinations: $3 \times 4 = 12$)

sufficient to make a beginning with a sample which is apposite to part of the theory or doctrine, for example, such as concerns Rogers' assertion that "where the self-concept is formed entirely from the evaluation of others, the individual will at some point face internal conflict."

We should add a word about disproving a theory. A theory that is built into a structured sample, of course, is not necessarily acceptable. Few believe in Jung's theory of introversion-extroversion any more, and one might well wonder what a structured sample of Jung's theory is likely to prove. A theory will usually be rejected when its testable propositions are not verifiable or have to be falsified; thus in the case of Jung's theory, if for a particular singular proposition none of the single effects A, B, C, or their interactions turns out to be significant as judged by F-tests, we shall have no support for the theory in this case. But a theory can be rejected even when every tested proposition supports it, as shown by F-tests and the like. Thus a typical proposition in Jung's theory is that introversion-extrover-

sion is no respecter of persons within the family: we can study *any* particular family in terms of the structured Jungian sample, along Q-lines, and show, indeed, that a particular father and mother are different and that some of their children are more like the one than the other parent, as Jung said would be the case. But what Jung thought was introversion-extroversion we might grasp, instead, as *identification* or the like in the family situation, and we would not be happy about the explanation in terms of introversion-extroversion. Moreover, even if every testable proposition that issues from Jung's assertions can be shown to be acceptable statistically (as we believe could be shown to be the case, for Jung was a keen enough observer of the human being), we could still reject the theory *in toto* and replace it with one that explains the scientific situation better, according to our judgment.

The mistake is often made of supposing that "large numbers of cases" are required before we can have such a theory or before it can be supported, or that, somehow, we are engaged in a process called "generalizing from the single case" when singular propositions are being tested. The truth is that we might prove a million testable propositions about a theory, and yet throw away the theory lock, stock, and barrel. Or we might test a theory in terms of a "single case" and accept it holus-bolus. Naturally, in the latter situation we might repeat the test, on another case maybe, but it would not really matter whether we did or could not, provided that the theory could be tested in other ways as well.

FURTHER CONSIDERATION OF PROPOSITIONAL SETS

Before leaving the subject of samples and their structure, we should return to give some further consideration to the kinds of propositions at issue in Q. Attention has already been drawn to the fact that two regions of propositions are always possible in Q-methodology—one with respect to variate designs, and the other to structured samples. The former is dealt with by factor analysis, the latter by variance analysis and small-sample doctrine.

Any structured sample involves many possible testable propositions having reference to the explanations at issue. Consider, for example, the simple design for two independencies, each for two levels (Table 8). The combinations provide the following sample: *a c*, *a d*,

$b\ c$, $b\ d$. Suppose this is replicated m times, the sample being $4m$ in size; the variance for any Q-sort for this sample is divisible as shown in Table 9. Depending upon there being significant "expectancies" or not, as judged by the familiar F-tests, for F_x, F_y, and F_{xy}, the following relationships are possible with respect to the *means* for components a, b, c, and d, respectively, in balanced design:

TABLE 8

Independencies	Levels		No.	D.F.
X............	a	b	2	1
Y............	c	d	2	1

(Number of Combinations: $2 \times 2 = 4$)

TABLE 9

	D.F.	"Expectancies"
ΣX	1	F_x
ΣY	1	F_y
ΣXY	1	F_{xy}
Σ replication	$4(m-1)$	1
Total	$(4m-1)$	

$$\begin{array}{ll} a > b & c > d \\ b > c & d > c \\ a = b & c = d \end{array}$$

There are 9 (3×3) arrangements of these of the kind $(a > b)$ *and* $(c > d)$. For more elaborate designs, of course, the possibilities become very large indeed.

The investigator, however, may predict which few of all the arrangements will, in fact, be found empirically and which not. Perhaps only two or three may be predicated. The confirmation or nonconfirmation of an investigator's explanations may have reference to these in particular. But if *others*, not predicted, occur significantly (either as well as, or to the exclusion of, predicted ones), then *falsification* is involved. That is, the asserted explanations are supplemented by or replaced by these others for which there is

significance as judged by F-tests. By the "falsification" of a proposition, therefore, we shall mean not "an alternative hypothesis" but "accepting one of the possible propositions that was implicit in the design at the outset." An "alternative hypothesis" would have reference to a new design altogether, for effects different from X or Y or both or for new "levels" introduced into X or Y or both in the above example. This use of the term *falsification* is narrower than is usual (92), but there are good reasons for it, and we shall adhere to it in Q-methodology in general.

APPENDIX I

Structured Sample (n = 80) for Jung's Theory

(The design has reference to Table 2)

a c e 1.............Silent, hard to understand.
2.............Seems reposed, inconspicuous.
3.............Unfamiliar things are met with apparent indifference and coldness
4.............."Still waters run deep."
5.............*Feels* God, freedom, immortality, or the like.

a c f 6.............Inaccessible, haughty.
7.............Thinking seems arbitrary.
8.............Works slowly, with difficulty.
9.............Impractical.
10.............*Theories* are important for him, *facts* not.

a c g 11.............Is guided by just what *happens* to him (impressionable).
12.............Benevolently neutral: the too-low is raised a little, the too-high lowered; the enthusiast is dampened a little . . . takes the middle line.
13.............Has little understanding of himself.
14.............Gives an impression of being perplexed or secretive.
15.............Impossible to say what will make an impression on him and what will not.

a c h 16.............Scents out new possibilities and pursues them without regard for himself or others.
17............."A mystical dreamer."
18.............Brusque.
19.............Everlastingly seeks a change in his "inner life."
20.............Reality doesn't seem to matter, fantasy does.

a d e 21..............Has a secret, concealed religiosity.
22..............Vain.
23..............Somewhat arrogantly ambitious.
24..............Can be mischievously cruel.
25..............Can be fascinating, because inscrutable, to others of opposite sex.

a d f 26..............Somehow believes in the "magical."
27..............Doesn't see when he is being exploited behind his back.
28..............Has a vague dread of the opposite sex.
29..............Breaks out with angry retorts against every criticism, however justifiable.
30..............."Keeps going" only by virture of inner struggle.

a d g 31..............Lacks self-judgment.
32..............Has a flair for the ambiguous and gloomy.
33..............There is something undermining about him, in contrast to apparent benevolence on the surface.
34..............Desires to dominate, but somehow craves to be loved.
35..............Dread of falling under hostile influences.

a d h 36..............Impulsive and unrestrained.
37..............Is underestimated and misunderstood.
38..............Can't understand why he is undervalued by public opinion.
39..............A "prophet."
40..............Always has the right answer.

b c e 41..............Feelings lack original warmth; gives an impression of posing.
42..............What he cannot *feel* he can't *think*.
43..............Displays extravagant feelings, which one can't believe.
44..............Follows the guiding line of his feelings.
45..............Loses himself in his love-objects, business, etc.

b c f 46..............Has lapses into short-sightedness.
47..............His thinking is banal, dull.
48..............His moral codes forbid him to tolerate exceptions.
49..............Ponderous.
50..............Unreasonable.

b c g 51..............Thoroughgoing realism characterizes him.
52..............His aim is concrete enjoyment, full actual living.
53..............Charming capacity for enjoyment.
54..............Dresses well.
55..............Aesthetic about sensual things.

b c h 56 ....Establishes the right social contacts.
57 ....Has a certain attitude of *expectation;* is always seeing *possibilities.*
58 ....Seizes new things with enthusiasm.
59 ....The present situation always tends to be a prison for him.
60 ....Consideration for the welfare of his neighbors is weak.

b d e 61 ....Opinions of incomparable tactlessness just seem to happen to him.
62 ....Prejudiced and depreciatory about those he loves.
63 ....Infantile, egotistical, profuse.
64 ....He breeds half-resolves.
65 ....Enjoys an excellent rapport with those around him, but hurts by utter tactlessness.

b d f 66 ....The end justifies the means for him.
67 ....Expressions are likely to be pointed, sharp.
68 ....Resentful, dogmatic.
69 ....Has little artistic sensibility.
70 ....Subject to nervous collapse, even if only mild.

b d g 71 ....Cold-hearted in criticism.
72 ....Reason is regarded as sophistry.
73 ....Superiority feelings.
74 ....Religion is considered an absurd superstition.
75 ....Somewhat captious and pettifogging.

b d h 76 ....Oversubtle in reason.
77 ....Apt to become "entangled" with unsuitable other sex.
78 ....Apprehensive about approaching illness or failure.
79 ....Doesn't mean to be inconsiderate, but just doesn't see things as others do.
80 ....Claims freedom from all restraints.

CHAPTER V

PSYCHOLOGICAL PRINCIPLES

INTRODUCTION

IF THE previous chapters have taught anything, it is that critical attention to some of the assumptions of psychology can be highly rewarding. Apparent truisms have proved to be fickle mistresses. We turn, therefore, to examine some of the psychological principles upon which Q-methodology is based, with an expectancy that here, too, discoveries will be forthcoming.

Reference has already been made to a number of psychological principles which characterize Q, or which it eschews. We began, it will be remembered, by proposing that our concern was to be with *behavior* as such, that is, with man as a concrete thinking and behaving being. Everybody knows in a general way that this is what psychology is about. Yet not everyone will agree that thinking is behavior. The matter, however, is of direct concern to us in Q. Q-technique provides a systematic way to handle a person's retrospections, his reflections about himself and others, his introjections and projections, and much else of an apparent "subjective" nature; and it is important, therefore, to have a clear notion about the scientific status of such matters. There are very many psychologists who regard themselves as perhaps the elite in matters of objectivity and impartiality, who will proclaim that these "subjective" matters are outside the scope of scientific method, if not *in esse* then *in posse*, because of the vagueness and speculation they involve and because of their essential unreliability.

We are afforded an excuse for dealing with these matters in critical detail, by taking issue with a neobehaviorist, Spence (151), upon what might seem to be a minor detail of his philosophy and about which we are sure that, upon reflection, he would not differ from us. It has to do with the use of the word "experience" as a basic postulate of behaviorism. It is the writer's belief, as well as that of some logical analysts, that all experience is behavior. Be-

haviorism, therefore, is all-embracing. But it has not been usual to think of it in this way, whence our present incursion into a prejudice that has so far denied to psychology its richest regions of scientific exploration and research.

BEHAVIORISM

Behaviorism purports to be an objective approach to psychology. By this is meant not merely that it is characterized by detachment and clear thinking, of the kind we so much admire in the scientist who, skeptical seeker after truth, believes only what he can prove. There is in behaviorism an implied opposition to *introspectionism*. The one deals with real things, it seems, and the other with mind.

But *isms* are at a heavy discount nowadays, and these issues have apparently long been outmoded in psychology. For has it not now become a science? The *Handbook of Experimental Psychology* (177), indeed, is a proud monument to this, and physiological and comparative psychologies clearly point to the same *objective* foundations. However, there are at least some misguiding protopostulations in it which, if not honestly faced, will continue to be misleading.

By "objective" psychology we shall mean, for the moment, psychology from the so-called "external" frame of reference. Behaviorism, too, took this stand. Both take pride in ridding themselves of subjectivity: a corpse would be more welcome at a wedding than anything subjective in a conditioning experiment or in any of the investigations pursued in the *Handbook*. It is true that the American self-psychologists and phenomenologists (100, 147) have offered to make inroads into this position from the "internal" frame of reference. There is clearly a division in the psychological ranks between the "externalists" and the "internalists," corresponding roughly to the biological and experimental psychologists on the one hand, and many clinical and social psychologists on the other. The former are particularly suspicious of anything subjective, whereas the latter accept a certain subjectivity but are not sure about its mentalistic implications. Already, for example, some are looking for *mind*, to be found if only psychologists look for it (147). In this situation, therefore, it seems extremely important to make clear the basic issue.

The truth is that it seems unprofitable to draw any distinctions

between what is objective and what is not, except in terms of dependable *operations*. According to logical analysis (209), so-called "subjectivity," when it is not confused with "mind," is merely an indication of undependability, variability, and absence of attainable "constant relations." The scientist is engaged in separating what is dependable from what is unstable or variable, and "subjectivity" is the name we give to the latter, the untamed horses of the scientific ranch. Similarly, it is unsound, except on grounds of convenience, to distinguish between the "inner" and "outer," "internal" and "external," frames of reference; certainly, there should be no imputation of the kind that the one is scientific and the other not, or that psychisms are necessarily implied in the one and real things in the other. There are, indeed, several other such frames of reference, as we shall see, none of which can have any prior claim to scientific regard. Moreover, it is unlikely that psychology, like the tides, flows at both high water and low—there must be some way to resolve the stark mass of psychology *without* a subject, that the *Handbook* epitomizes, and psychology *with* a subject, that the modern self-psychologists are searching for.

Allied to the "external" standpoint and to *objective* psychology is a deep suspicion of *verbal report*, a term we extend to cover man's ruminations, reasoning, retrospections, his "inner thoughts," and the like, no less than what he says or writes. No FBI sleuth could be more suspicious of things communistic than present-day experimental psychology is of verbal report: for who can believe what anyone *says?* Only a bald "Yes" or "No" is acceptable, as a response to "stimulus instruction." Yet we may learn much, however, precisely by *dis*believing such report. Moreover, Hunter (84), Kantor (88), and Skinner (144), to name only a few behaviorists, did not reject verbal report on protopostulatory grounds but merely provisionally or pro tem, because it was not at that time amenable to reliable operations. But the situation is very different now. Dynamic psychology has provided masses of acute observations about such behavior and certain broad theories for its understanding; and Q-technique can lend to these many operations that bring to light the necessary dependable relations. Fisher's experimental methodology is now as applicable to Freud's analysis of "Dora's dreams" (71) as it is to agronomy. Factor analysis, properly regarded as a matter of

dependency analysis (like Fisher's) and not as a domain of interdependency analysis from which "unitary" factors have to be extracted (93), is as applicable to man's wishes, hopes, attitudes, delusions, beliefs, and all else, and for "single cases," every bit as much as it (factor analysis) is to *mental tests*. Even the ghostlike *self*, the despair or hope of psychologists and philosophers since the time of Plato, is now amenable to Q-technique procedures. These operations, however, in no way mean that anything other than *behavior* is at issue; but the term has to be such as to encompass all operations, from any "frame of reference," whether "inner" or "outer" or any of the others we are to define below. The rigid postulation of the primacy of the "external" standpoint, upon which objective psychology has been raised, has done its best, we believe, to sink the ship of behavior—a few more "Handbooks" will indeed sink it deeply and irretrievably. But we have no wish to raise the same ship, balloon-like, into the high heavens, with a false subjectivity. In between these extremes there is a sea vast enough for every scientific maneuver and upon which true behaviorism, properly built, can be safely launched and steered.

Metaphors aside, we have a purpose, therefore, in taking issue with Spence (151) upon the following seemingly slight and trivial matter.

SPENCE'S POSTULATE

Spence's article (151) begins with the statement that what Watson stood for is deeply imbedded in American psychology. We agree but believe that it has not yet fully grasped Watson's more essential importance. From the systematic standpoint, Watson marks the beginning of scientific psychology, not for any discoveries he made or techniques he developed, nor for his insistence upon an "external" frame of reference, but because of his sudden grasp of the irrelevancy for psychology of the age-long existential and functional *mentalisms*. In this he guessed correctly, without the sophistication of modern logical analysis to support him and in spite of the widespread heresy, *scandalum magnum*, that it seemed to involve. The radical nature of this guess can be appreciated, perhaps, only by the generation of psychologists who were bred upon the mentalistic schools, and it is easy to overlook its significance in the modern rush of fact-finding. It is probable, indeed, that most of the contributors

to the *Handbook of Experimental Psychology* still believe in "sensation" qua "experience," as a possible scientific issue. Spence, too, it seems to us, has not firmly held to the nettle of Watson's methodological discovery.

As a first principle, Spence supposes that the concern of behaviorism is to "bring order and meaning into the realm of certain events provided by immediate experience" (151). This immediate experience is taken for granted in all the sciences (Pratt, 124); according to Boring (29), it is the background or the "dator" of all scientific data, which, again according to Boring, is "unrealizable as reality except inductively." We believe, with others, that these assertions are not relevant to behavioral science (63, 134).

It will be objected, perhaps, that scientists must begin somewhere, and this innocuous first postulate seems reasonable. But it implies, where it does not directly invoke, a belief that "experience" and "behavior" are different matters (63). Perhaps Spence did not wish to imply anything of the kind. Or it may be that we are attributing to the word "experience" what no American psychologist would ever dream of doing. We were bred upon the mentalisms of Spearman, Ward, and Stout and perhaps have not discovered that Spence had no need to be cautious in his first postulate, because he and his colleagues had thrown all mentalistic conceptions into the high heavens long ago. However, to judge by Kantor's efforts or by John Dewey's notions about "experience," there are many reasons to doubt whether all is really as straightforward as this. The prejudice in the direction of "experience" qua "experience" is deep and protopostulatory to our culture; some questioning of colleagues, moreover, leaves the position as we supposed it to be. Watson's guess has not really been thoroughly grasped. The following statement from the leading psychologist, Adrian, we are sure, will find a sympathetic echo in many an experimental psychologist's and every cyberneticist's heart:

> Yet there remains the formidable problem of the intervening events. The human mind comes in somewhere in the chain of causation between the physical events in the sensory and the motor pathways [4].

Logical analysis indicates very clearly, however, that "experience" and "mind," in any existential sense, are fictions and that, although there are plenty of problems, there is no formidable one of the kind

implied in Adrian's remark. We have *behavior* to study; that is all. "Statements about mind or consciousness," writes Mace (111), "turn out to be, on analysis, statements about the behavior of material things." Farrell (63), Gilbert Ryle (134), and others have the same to say about the mentalistic fictions of psychologists, a standpoint long sustained by Kantor (88).

However, "events" having been postulated by Spence, we may wonder what he proposes to do about them. He contrasts the familiar intra-organic or "internal" approach, and the extra-organic or "external." He does not remind us, however, that there have always been two main streams of endeavor with respect to the internal standpoint: apart altogether from what it could mean in relation to the central nervous system or other bodily organs, it led to the study of mental structure (existentialism) and of mental *function*. The English empiricists—the Wundts, Titcheners, down to the Borings and the Bertrand Russells of today—have made special efforts to elucidate the mysteries of mental structure. The wide sweep of "romanticism,"[1] from Kant to Brentano and so through to disciples of the Würzberg school, besides Stout, Ward, and Spearman, had concerned themselves royally with mental *function*. Spence rejects the *functionalists*—at least he refers to the faculty psychologists in this connection. He also rejects the structuralists, in so far as he makes reference to their method of introspection, through which, alone, "experience" was supposed to be "realizable as reality." He has to say, for example:

> If we can judge from the interminable disagreements of the introspection psychologists themselves, this class of experience (intra-) does not meet too well the requirements of social verification and acceptance demanded by the scientists.

Both the structural and the functional objectives are discarded, then, even if only on grounds of unreliability. Therefore, argues Spence, the *external* standpoint alone is acceptable—even though the first postulate concerned "experience."

The argument that, because introspection is unreliable, we must therefore give up the internal framework is a cliché of the American

1. The term, of course, is usually associated, in psychology, with the stream that begins with Rousseau and finds its most turbulent course in some German psychologies; but, as we can now see it, from Kant through Hegel to Brentano and all that stems from these, "romanticism" is as good a name for all of it as any.

behaviorists. The logical position, however, is merely that introspection proved unprofitable, undependable, and generally unreliable; therefore, *either* the method was inadequate, or its objectives chimerical, or both. These are not arguments against the internal standpoint as such. Nevertheless, it was thrown overboard, along with introspection: only certain hankerings after "experience" remain, like ghosts in the closet, gently to plague the Spences. All behaviorists, on such flimsy grounds, proceeded to study the behavior of *others* exclusively, other persons, rats, dogs, pigeons, beetles, worms, and hens, but never *themselves*. Thus the fine analytical essays of James Ward (97) passed unnoticed or neglected when he grasped, long before his time, that along such lines psychology would necessarily become a science without the "subject" it everywhere implies.

Introspection was brought into disrepute because it was used as the means to an impossible end, namely, to discover "sensations" and "experience" as existential psychic matters, in a world apart from things; and certainly it is time to put this "experience" out in the cold of nonscience. But it is another matter to discard the internal frame of reference along with it. It is as easy to experiment with one's self as with any rat or dog; and, although we reject any concern with "mind," it is quite simple to study ourselves in all our psychological interactions, including our "subjectivity," which is, indeed, perhaps the most characteristic of all of which we are capable. That we reflect, retrospect, and the like and that many dynamic matters occur of which we are not cognizant is certain. What has been lacking has been a proper method for their study. Spence, in the aforementioned article, returns to the straight behavioristic pathway when he asserts that verbal report, like all movements, is essentially "extra-organic," to be studied like any other behavior. It should be studied, he writes,

> . . . as a base from which to infer theoretical constructs which presumably represent internal or covert activities of their subjects.

Here again there is perhaps a lingering-on of the old melody, that there are covert activities inside the person. Again we doubt whether Spence meant what this statement could imply to the incautious; but he offers no explicit assertion that "internal" means merely "from the personal standpoint," or "as I see it," or "as I suppose it

to be," or any other of hundreds of such dispositions, and not anything covert in any mentalistic sense.

Many other behaviorists, of course, including Skinner (144) and Hunter (84), reject verbal report on grounds of dubiety or variability. Skinner discarded the vernacular from scientific concern because of its supposed complexity and dubieties of meaning, not because of any supposed "subjectivity" qua psychism or the like. Hunter cautioned against the use of "verbal methods" until behaviorists "are better able to evaluate them"—but, far from rejecting them, they became, in his anthroponomy (84), embodiments of the environment. We believe that along Q-technique lines there now exist methods for the "evaluations" that have so long escaped us. Only the identification of "inner" with mind and of mind with unreliability has led to the rejection of subjectivity as worthy of our scientific endeavors. It is, instead, likely to be the central issue in all psychology.

We should add a word, however, about the modern phenomenologists, self-psychologists, and contemporary social psychologists who equate social events with "perception" (100) and postulate a person's "private world" of experience. They approach psychology from the "internal" frame of reference. It is safe to say that any experimental studies that these psychologists care to pursue can proceed without their postulates of "experience," "phenomenology," or "private worlds"; what is essential is the acceptance of the "inner" frame only in the behavioristic sense of a person's self-reflections, his self-conceptions, his self-observations, unreflective or otherwise, as worthy of true empirical study and as likely to be as rewarding as anything that has issued from the concern of psychology with *others*, studied from the outside. Objectivity is by no means the special privilege of the *other*-psychologists, and the self-psychologists (147) have been correct to assert this. But care is needed lest the "percepts" become "experience" as such; Snygg and Combs (147) argue for a *mind* as such which can be *found* if only psychologists look for it; they, like Spence, will no doubt agree that they had no mentalistic discoveries up their sleeves. But Krech and Crutchfield (100) impale their social psychology on untestable *instants* of time, a fiction that could come only from taking "perceptions" literally as ghostlike flashes of a substantive kind. What these psychologists are con-

cerned with, unless we are mistaken, is that an individual has his own "standpoint," his own "conception of things," and a hundred and one such dispositional propositions about his behavior. But they had to support a viewpoint that ran counter to the prevailing dogma of the objective *other*-psychologists, and they clutched too eagerly, perhaps, at the ghosts of phenomenology. The really important lesson remains, however, that studies can be made objectively within the so-called "internal" framework.

Thus we have no place for Spence's first postulate. Nor can our concern be limited in any way to either the "external" or the "internal" frame of reference.

REPRESENTATIVE PROBES INTO BEHAVIOR

There are more "frames of reference" than the "external" and the "internal." Before defining them, it is as well to look back a short while into certain critical analyses of behaviorism. The systematic approach to the study of behavior should start, we believe, from the simple definition of *segments* of behavior,[2] such as Kantor (88) long ago postulated as fundamental, or as Skinner (144) has adumbrated more recently and more narrowly. Kantor, as well as A. F. Bentley (21) and recent offshoots of John Dewey and Bentley in the *transactionism* of Cantril and others (45), all in different ways hold that such behaviors are rooted in some kind of interaction. This, however, need not concern us in detail; instead, we would draw attention, very briefly, to certain protopostulatory matters at issue, such as Bentley (21) first attended to, about the spatial and temporal suppositions of behavioral segments. When Lewin (104) proposed to represent these matters in a formal manner, as he very notably tried to do, the space into which a person's experiences were placed was isolated and separated from the space in which he moved among things and other persons. Lewin's doctrine, in this regard, eventually led to an "encapsulation" of psychology within each person, as Brunswik (35) described it. The modern self-psychologists also posit "private worlds" in everyone, as postulates, with a world of real things around. Everywhere, indeed, these spaces or worlds or the like are isolated from one another by impassable barriers placed there by definition, implication, or postulation. Kantor (88), although he

2. See note to p. 24.

perhaps ignored the behavioral counterpart of everyone's personal reflections, grasped the need for a monistic space, the same for all empirical propositions. It was A. F. Bentley (21), however, who argued more particularly that psychology was unlikely to progress far until it could formulate or accept nonisolationality as its basic postulate in this matter of spatial representation. This is clearly in line with a major outcome of logical analysis, that the concern of psychology is with testable propositions about behavior (92); for if there are no psychic involvements, there is no need for isolated "worlds" or "spaces"; and, by the same token, it is hard to maintain that behavioral spaces need to be segmented (21). Moreover, any implication that a segment of behavior is an isolated region would be wrong; it is merely studied *in situ*, much as a mountain might be surveyed apart from the land mass of which it is a part.

It is from this standpoint that we regard behaviorism, properly understood, to mean the study of behavior from any scientific vantage-point, for nonisolated spaces. The concept of *time* we find easily met, if the mistake is not made of supposing that predictions are to be asserted about "fields" at an *instant* of time, a trap into which Lewin, Krech, and Crutchfield and many modern self-psychologists have fallen (147) because they falsely postulate the *historical* and the *ahistorical* in psychology, by analogy with special situations in physics. Behavior is always rooted in history, as Kantor (88) so succinctly put it. For us the "ahistorical" is the duration of a behavioral segment, about which scientific laws are formulated and predictions made, however long or short the segment may extend in the clock-maker's time.

We might suppose that behavioral or interactional segments, then, have a certain concreteness in time and space into which soundings or probes can be made through some of their natural cracks or crannies. The long history of psychology has to suggest that there are three main cracks of the kind. We might look at a segment (*a*) through the eyes of the "inner experience" (in the popular sense of the words) of the interacting person or persons involved; or (*b*) the "outer" performance as such of these persons may be attended to, for they are there for everyone to see; or (*c*) the historical connections of the segment may be studied, ramifying as they do into and out of the person or persons and things concerned.

Each, however, can be further subdivided in such a way as to provide a more complete and representative coverage of the possibilities. These we call *representative probings*, for they deal with a behavioral segment from all its more obvious crannies. The set of probings into any segment is, briefly, as follows, where the interactional setting may be regarded, for purposes of illustration, as involving a person X, who is observed by another, Y, who might be a psychologist:

a) "Inner"
- (i) Psychism *of* X
- (ii) Self-reflection *of* X
- (iii) Reconstruction of ii *by* Y

b) "Outer"
- (i) Observations of X's behavior *by* Y
- (ii) Self-observation of X's behavior *by* X

c) "Historical"
- (i) By way *of* X's self-reflections or self-observations
- (ii) By way of Y's outer observations *of* X's behavior

These, admittedly, are clumsy designations, but we have failed to find any better names for them. They apply to all psychology in which persons are in interaction, individually or in groups. The various systems or schools of psychology, down the ages, have favored one or the other of these probing points, and, up to now, none has ever approached psychology from the standpoint of all of them contemporaneously. Thus, introspectionism narrowed itself to *a* (i); classical behaviorism sees everything by way of *b* (i); psychoanalysis restricts itself to *c* (i); and Gestalt psychology deals with *a* (ii), the probe of so-called phenomenology. Kantor's interactionism involved clear recognition of *b* (i) and *c* (ii). The approaches *a* (i) and (ii) and *b* (ii) are roughly what experimental psychology has dealt with as the method of *impression*, whereas *a* (iii) and *b* (i) have their counterpart in the method of *expression*. Obviously, *a* (ii) and *a* (iii) are what many present-day self-psychologists rely upon, although, like Snygg and Combs, they confuse *a* (i) and *a* (ii), regarding *a* (iii) as the place for scientific operations. Extraordinary prejudices and mistakes attend the present-day psychologist's regard of these probing points. The neobehaviorists, such as Spence, reject *a* (ii), *a* (iii), and *b* (ii), because they confuse them with *a* (i). Many personality psychologists throw away *a* (ii), and others *b* (i), for no reasons better than prejudice. Few, except the psychoanalysts and a few

sturdy geneticists who work by way of *b* (i), ever attend to *c*. It is clear that *a* (ii) and *a* (iii) are what many present-day psychologists are concerned about as the "personal frame of reference," whereas *b* (i) and *b* (ii) represent the "outsider's frame." But when we provide suitable behavioral segments for our study (and even the physicist had to learn that some physical events—the electrical—were more pregnant for their purposes than others, such as heat), it becomes apparent that all the regions, except *a* (i), are all within one and the same operational frame of reference.

An example may help to make this clear. It can be highly schematic only, but it will be of interest for the reader to expand upon it, perhaps, after he has read the rest of the book. Consider one of the *dreams* referred to by Freud (71) in his account of his patient Dora. The dream was a concrete segment of behavior. Freud shows, however, by way of the method of association, that it had its roots in a particular series of events in Dora's life, spread over a lengthy period of time. He explains the dream as the outcome of conflict in relation to these events. Psychologically, the dream was a caricature, or subsumption, of these events, as well as the expression of an immediate conflictual situation. How, then, are we to study such a concrete segment of behavior with its lengthy associated attachments and its immediate outburst of conflict, as represented by the dream? Can we put Freud's hypothesis about Dora's dream to an empirical test? We shall show later how such problems can be dealt with along Q-technique lines. Meanwhile, it is sufficient to say that Dora could have given us a large number of self-referent statements, all having some connection with the series of events at issue; these could have been collected in the way described in chapter iv. A Q-sample could have been composed from these, structured with respect to Freud's hypothesis about the case (see p. 250). Various Q-sorts could then have been undertaken both by Dora and by Freud, under many different conditions of instruction or experiment. These could have been by Dora herself—a matter of probe *a* (ii)—and by Freud, who could have tried to put himself in Dora's shoes, to describe, if he could, whatever Dora might be expected to do at *a* (ii)—a matter of probe *a* (iii). From the "external frame," Freud could operate with the same Q-sample from his theoretical framework, as an observer of Dora's behavior—a matter of *b* (i). Dora herself could try to put

herself into Freud's shoes and repeat whatever he does at *b* (i)—a matter of *b* (ii). Similarly for the historical probes *c:* Dora could have provided Q-sorts from her standpoint about the sequence of events, as could Freud from his—a matter of *c* (i) and *c* (ii), respectively. We have not said what the various conditions of instruction would have been, since detail would be essential for these. Probe *a* (iii), however, would have dealt with the possibilities of *countertransference*[3] on Freud's part, which he overlooked; and *b* (ii), likewise, could have concerned itself with the *transference*[4] situation, which Freud neglected somewhat in this case. The sample itself would embody psychoanalytic theory in balanced block design, in terms of Dora's self-referent statements, and its construction is the investigator's concern. Dora's Q-sorts would have brought psychoanalytic mechanisms to light objectively, i.e., from operations performed by Dora herself. The historical variates would have touched upon particular events, considered by either Dora or Freud to be significant. All this would relate directly to the dream, with the one Q-sample in use for all the operations by Dora and Freud. All the operations would be capable of resolution in factor and variance terms, i.e., every detail is capable of comparison with every other detail, for all six probing points.

This will seem a vast undertaking; but similar studies, involving fifty variates or more, have been undertaken. Ordinarily, one would deal with such a segment of behavior in a less comprehensive manner, probing into it now here and now there for different theoretical reasons. TAT or similar techniques may become involved in the inquiries, as matters of *b* (i) probe. Our point, however, is that, in principle, something can be done about any behavioral segment, if we try, from all six probing points. This can be for all six simultaneously employed, as in the above example. The important matter in the latter case is the possibility of comparing data gathered from all six vantage points in a directly operational manner. Ordinarily, Freud and the psychoanalysts, apart from providing no operational procedures and testable propositions, have looked at dreams like Dora's from the external frame of reference *b* (i) and *c* (ii), respec-

3. That is, the way in which an analyst becomes emotionally attached to his patient, without being aware of it.

4. The patient, in this case, is attached emotionally to the analyst.

tively—their data are what they observe as they psychoanalyze their cases. The method of free association, for example, consists of Dora associating but Freud putting two and two together. It is true that Dora would put two and two together as well at times: but the psychoanalyst never refers to this from his patient's standpoint. Freud, for example, in the paper concerning Dora's dreams (71), does not refer in a single instance to any of Dora's self-reflections; he provides several, on the other hand, about himself.

No doubt all is a matter of emphasis. But that psychologists have tended to restrict their endeavors, in the main, to one or another of these various probing points into behavior can scarcely be denied. It is against artificial restrictiveness of this kind that Brunswik (35) has been directing his attention. Here, in the six probes, is a vast example of it, involving whole systems of psychology. It is, of course, likely that some behavioral segments are more open to one probe than another; but all the six vantage points, *a* (ii) to *c* (ii) above, that is, excluding *a* (i), are open to the full flush of scientific procedures, and there can be no a priori reason for restricting attention to this or that probe, regardless of the others. It is doubtful whether any *systematic* psychology or a reasonably comprehensive *theoretical* psychology of behavior can ever be possible as long as different systems of psychology impose artificial restrictions of the above order upon their science. We believe that what is intrinsic to Kantor's *interactionism* requires all six probes *a* (ii) and (iii), *b* (i) and (ii), and *c* (i) and (ii) to be admitted as basic to it. It is indeed not an accident that Kantor had these within his grasp, that Bentley foresaw their existence, and that we can see the possibility of a behavioral science which will offer its favor to none of these regions, on a priori or protopostulatory grounds, at the expense of any other.

We would repeat, however, that in all except *a* (i) the concern is with *behavior*. The science of behavioral segments, covered from all six vantage points if need be, might well be called "behavioral science," to borrow the term from our colleague, Dr. J. G. Miller.

CONCLUSION

Spence, we hope, will forgive us if we have made too much of his first postulate. There is no need, however, to oppose *behavior* to nonexistent "experience," and existential or functional psychisms are

nowhere our concern. Nor can subjectivity and objectivity be placed in opposition, except in common parlance to mean different *attitudes;* otherwise, having repudiated mentalisms, all that distinguishes these is the possibility or not of having testable operations. Nor can arbitrary restrictions be placed upon the representative probing points or "frames of reference" through which, *in principle*, it seems possible to study any segment of behavior.

However, although behavior can be studied from all these vantage points, there is perhaps a special responsibility to deal with the "inner" standpoint. It would take us too far afield to argue the issue, but it is possible that the "external" frame of reference can reach only into physiological processes or the like which are *postulatory* to the more essential problems of psychology: this is not to say that they are not worth studying, but only that advances of knowledge about them will not necessarily help us to understand the real problems, any more than additional knowledge about the *rules* of chess, such as how they were initiated or who made them, will be of any help to the chess player. The problems in psychology are per-
haps like the *games* of chess, not their rules. What seems important is
concrete behavior, including the concrete subject (197), as Ward long
ago supposed and as Kantor (88) has been saying, almost alone, for many years. While there is no place for "experience," phenomenol-
ogy, "private worlds," and the like, there is plenty for the essential objectives of those psychologists who believe that one's yearnings, wishes, ruminations, reflections, wantings, inclinations, fancies, dreams, remembrances, and a thousand other "inner" forms of be-
havior are of crucial importance; and we believe that most of these matters can now be brought into testable propositional form. They can be dealt with as objectively as any psychologist ever dealt with a rat.

CHAPTER VI

SOME STATISTICAL AND EXPERIMENTAL PRINCIPLES

INTRODUCTION

EXCEPT for a few further principles of particular importance to Q-technique, all is now in readiness for its practical applications. We offer no account of either the statistical methods of factor analysis or of variance design and small-sample theory. For the main part we use the centroid method of factor analysis, as described in detail by Thurstone (188). No very adequate account of the Fisherian methodology seems to exist, but Fisher's own work (68) and accounts of experimental design by Brownlee (33) and many others are widely available. Of more particular reference to Q-technique is a recent work by Cronbach and Gleser (55), who give an admirable account of the statistical methods employed in studying the similarity between persons in *profile analysis*.[1] They discuss Q-technique, in particular, with considerable point. However, these authors assume that individual differences and the necessity for concise measurements and scales are essential and axiomatic to all else; and, since they compare Q with other methods of analysis which certainly depend upon such assumptions, whereas Q does not, the relevancy of their work for Q is not complete. Q is not a method of profile analysis. Our *system* (3) of the 1936 *Psychometrika* paper (171) is, however, since this has reference to R-methodology principles. The latter principles are *protopostulatory* in Cronbach and Gleser's work; that is, they are assumed without question and without recognition that they might be questionable. But this is true, also, of course, of all Burt's (41) references to Q-technique, as well as of Babington-Smith's (12) and Cattell's (50).

1. That is, many *measurements* are made about several persons, using standard scales and tests. Many statistical procedures can be applied to these in such a way that one person's arrangement of scores can be compared with any other's. The concern is said to be with *profiles*. Clearly, the measurements are defined, in the first place, by way of individual differences.

There are one or two statistical matters, however, which are of particular interest to us in practice, some understanding of which can help the expert, at least, to see what we are doing at some points in the studies with which we are to be concerned in the following discussion. It will have occurred to many, no doubt, that there must be some necessary relationships between the *factors* reached by way of factor analysis and the *effects* represented in structured samples. We have so far maintained that we are involved in Q-method with two sorts of facts and propositions—those dealt with as *factors* and those dealt with as *explanations* for structured samples. It is best to maintain this separation, since many propositions can be tested by way of factor analysis without reference to the structure of their Q-sample. Likewise, variance analysis can be pursued in relation to the structure of a sample, without reference to any *factors*. But under certain conditions the same facts can be reached both ways, by factor analysis of the variates and by variance analysis and small-sample treatment of the samples. Even so, as we shall see, the factor analysis of data may have interesting inductive possibilities not available to the variance analysis. We shall disregard these for the moment, however, and proceed to consider merely the formal relationships that can exist between *factors* and *effects*.

CONDITIONS FOR FACTORIAL DESIGNS

The concern is with the relationships between the variance analysis of single arrays, for balanced designs (Fisher, 68), and the factor analysis of several. Under certain conditions, *effects* A, B, C, . . . , for a single array in balanced design, can have corresponding factors α, β, γ, . . . , in dependency factor analysis. Orthogonal factors and effects are assumed.

It is helpful, in the first place, to have the statistical and other conditions of balanced designs in mind. They are as follows: In the elementary analysis-of-variance situation, data are available in r rows and c columns, and the individual values are supposed to arise from *general, row, column,* and *cell* effects. It is assumed that the effects are *additive* and *uncorrelated.* The analysis-of-variance procedure leads to mean estimates (so-called "unbiased" estimates) of the "population" component variance for the row, column, and cell

effects. When tests of significance (F-test [68]) are at issue, it is assumed that the variances of cell effects are the same and, jointly, are normally distributed, so that all cell effects are independent. That is, error estimates are at issue. It is customary, of course, to test experimental data for its "homogeneity" in the latter respects (Fisher, 68; Brownlee, 33).

Balanced factorial designs have these conditions in attendance, along with others to which Fisher had always drawn attention (68), namely, those concerning the broadening of the logical basis of inferences, the elimination of certain large sources of error, and the particular estimation of error precisely where it is to be expected operationally. In the latter connection it is perhaps not sufficiently recognized that Fisher's procedures are closely tied to achievable operations, such as the logic of modern science requires. It is important, also, that Fisher's procedures are matters of *dependency* analysis, that is, the postulation of independent variables and the study of their dependent effects are always at issue. The concern in factorial designs is with effects $A, B, C, \ldots$, for $m, n, p, \ldots$, levels, respectively. These main effects have $(m - 1)$, $(n - 1)$, $(p - 1)$, $\ldots$, degrees of freedom, respectively. The AB interaction has $(m - 1)(n - 1)$ degrees of freedom, and so forth for all other interactions. Interactions correspond to *unspecified* effects, i.e., present in spite of the specification of all main effects as A, B, C, . . . Thus in experimental work we usually do not anticipate that interactions will be significant. All effects are orthogonal, and any component of an effect is also orthogonal to any other factorial effect (68). But components of the same factorial effect may or may not be independent. All these various definitions, purposes, assumptions, and consequences of factorial designs are well known (33, 68) or are dealt with in the various texts on the subject.

In the situations we meet with in Q-method, all these matters are constantly at issue. For our present purpose, however, we would draw attention to the following particular principle. Consider the balanced factorial design in Table 1. The *same* design may also be written as shown in Table 2. In the former, effects A and B are orthogonal by definition; in the latter, although A *and* B are orthogonal to C, it is *also* the case that a and b may be orthogonal to c and d.

There is no such warranty, however, for effect C of Tables 1 and 2. It may be that the levels of C are linearly related, or it may not be so. If effect C, however, is divisible into *two* orthogonal effects, such as is the case for A *and* B of Table 2, the factorial design may be as in Table 3. But there are also other possible arrangements of the four levels for such components, namely, *eg*, *fh*, and *eh*, *fg*.

TABLE 1

Effects	Levels				No.	D.F.
A	a	b			2	1
B	c	d			2	1
C	e	f	g	h	4	3

(Number of Combinations: $2 \times 2 \times 4 = 16$)

TABLE 2

Effects	Levels				No.	D.F.
A *and* B	a	b	c	d	4	3
C	e	f	g	h	4	3

(Number of Combinations: $4 \times 4 = 16$)

TABLE 3

Effects	Levels		No.	D.F.
A	a	b	2	1
B	c	d	2	1
C_1	e	f	2	1
C_2	g	h	2	1

(Number of Combinations: $2 \times 2 \times 2 \times 2 = 16$)

It happens that, in the situations for Q-technique, effects such as A and B above, for two levels each, can be represented by corresponding orthogonal *factors;* an effect such as C, however, may or may not give rise to one *factor*. It could give rise, instead, to several. Under certain dependency conditions, the impossibility of breaking an effect such as C into orthogonal parts C_1 and C_2 is proof of the "primary" nature of effect C. These matters are attended to below.

RELATION BETWEEN FACTOR ANALYSIS AND VARIANCE DESIGN

Balanced effects A, B, C, . . . , for an array in Q-technique can correspond to factors A, B, C, . . . , in *dependency* factor analysis. Interaction AB would be represented by such a factor, too, which was not specified at the outset. Orthogonal factors are assumed. A simple proof of these matters is as follows:

In Q-technique the concern is with N statements (verbal statements, traits, art objects, or the like, of a balanced design) which are scored by or about a person. Each set of N scores constitutes a Q-array. Let us suppose there are several such arrays, p, q, r, . . . (such as Q-sorts made by different persons p, q, r, . . . , or by the *one* person under different conditions p, q, r, . . .). The score w_i given to a statement i in any such Q-array may be written as follows:

$$w_i = \alpha a_i + \beta b_i + \gamma c_i + \cdots , \tag{1}$$

where α, β, γ, . . . , are factor coefficients for factors A, B, C, . . . , respectively, in the factors, in standard terms. Factors are uncorrelated, and $\alpha^2 + \beta^2 + \gamma^2 + \cdots = 1$.

Two forms of analysis are then available to us, since the sample N is structured. Each array of w-scores can be analyzed by variance method, or the several arrays can be correlated and factored.

If the N expressions of equation (1) are written for the N statements of a single Q-array, two-way tables can be formed for each *pair* of effects at a time. That is, a suming the ideal situation that factor scores are standardized and normally distributed (although for our purpose other symmetrical distributions are acceptable), each *factor-array* may be dichotomized into scores above and below the *mean*. Each would contain $N/2$ observations, and the *means* of the two halves would be equal but opposite in sign. Any two-way table for *pairs* of factor scores is thus doubly centered: the means for its rows and columns would be equal, and equal each to zero, since the factors are uncorrelated.

However, this is not the case if the factor scores are weighted by their respective factor loadings. The variances of the factor-arrays, so weighted, become α^2, β^2, . . . , respectively. If we now form two-way tables for the factors in *pairs*, it is clear that significant effects will be possible.

Thus, consider A, B, each for two levels a, b, and c, d, respectively.

There are four combinations of these, *ac*, *ad*, *bc*, *bd*. In a structured sample each would be replicated m times, providing a sample $N = 4m$. For simplicity, suppose $m = 1$, $N = 4$. We have, then, a sample of 4 statements, whose scores in factor terms may be written as in Table 4. Table 5 is the two-way table.

TABLE 4

Statements	Factors	
	A	B
ac	αs	$+ \beta s$
ad	αs	$- \beta s$
bc	$-\alpha s$	$+ \beta s$
bd	$-\alpha s$	$- \beta s$
Means	0	0
Variances	$4\alpha^2 s^2$	$4\beta^2 s^2$

TABLE 5

	A		Means
	a	*b*	
B *c*	$(\alpha s+\beta s)$	$(-\alpha s+\beta s)$	βs
B *d*	$(\alpha s-\beta s)$	$(-\alpha s-\beta s)$	$-\beta s$
Means	αs	$-\alpha s$	

The variances of the row and column *means* are 2 $\alpha^2 s^2$ and 2 $\beta^2 s^2$, respectively. These are proportional to the variances of the *factors* α^2 and β^2, respectively. If the sample has m replications, $N = 4m$, each s in the above argument is merely the *mean* of the factor scores for each *half* of the appropriate factor-arrays. Since A and B are uncorrelated, these must be the same in amounts.

Thus the *factors* in the one case can be the main *effects* in the other, and vice versa.

The only important difference between the *factor* and *variance* situations, operationally regarded, concerns the way in which error estimates are made. In factor theory the significance of factors is made to depend *not* upon an operationally defined error variance as such, but upon large-sample theory and the consequent standard

error expressions (188). In variance design the error variance is based upon immediate operations, for the "cell" variances, with the principle of randomization in mind.

It remains to consider how the correspondence between *effects* A, $B, \ldots$, for each array in turn and *factors* $A, B, \ldots$, for a $(p \times p)$ table of correlations for p-arrays is reached in practice. It is achieved by *dependency* factor analysis. The proof of a "primary" factor, to which reference was made above, also depends upon *dependency* analysis being at issue.

SIMPLE AND SIMPLEST STRUCTURE

The notion of *simple structure* is now well known in factor analysis. It is Thurstone's way of dealing with the gross permissiveness of centroid factors, and mention of it has been made earlier (p. 41). The structure is usually reached, in practice, in relation to correlated factors, and only rarely for the original orthogonal ones of the centroid analysis.

We propose to use the term "*simplest* structure" to refer to simple structure for orthogonal factors. The effects in balanced block designs involve the assumption of orthogonality, and we wish, of course, to make direct comparisons between *factors* and *effects*. For this reason we always pursue our *dependency* forms of factor analysis (as described in chap. ii) in relation to orthogonal factors. That is, if we do not know the structure of a Q-sample beforehand or if it is an unstructured sample, the factor analysis is conducted with the expectancy that any explanations we may offer for the data should be representable, in the sample at issue, as a balanced block design. A solution to the rotational problem is looked for which could have come from a balanced design in the sample. There are methodological difficulties about the concept of simple structure, to which attention has already been given (p. 36). But the correspondence between *factors* and *effects* brings to light other limitations in the use of the notion of simple structure. In variance designs the main effects may be X, Y, and Z, and each of these may be significant; but any of the interactions for them may also be significant, i.e., XY, XZ, YZ, XYZ. If all were significant, main effects and interactions, seven factors would be necessary in factor analysis, to correspond to them—supposing again that the factors are orthogonal. In experimental work we are usually interested more in main effects than in

interactions, although interactions are, of course, important as evidence of unspecified effects, i.e., other than the main ones which were specified in the design. Factor analysis provides us with no way of distinguishing between the importance of its factors.

Again, the search for simple structure has led to interest being centered on tests which are "pure" with respect to a factor, that is, which are loaded in that factor and in no other. If there are three factors, A, B, and C, any variable has the following eight possibilities with respect to them:

1. A alone may be significant,
2. B alone may be significant,
3. C alone may be significant,
4. A and B may both be significant,
5. A and C may both be significant,
6. B and C may both be significant,
7. A, B, and C may all be significant,
8. *None*, neither A, B, nor C, may be significant.

(i)

Classes 1, 2, and 3 are usually called "pure" for the factors, and 4, 5, 6, and 7 "mixed." These various possibilities will not occur, of course, with equal frequency for any set of variates of a study. Thus there may be many variables of one class and few of another. It has become almost axiomatic in factor analysis to regard the "pure" cases as of central importance, so that the concept of simple structure is usually applied to the situation represented by classes 1, 2, and 3 only, whether for correlated or for orthogonal factors. It seems important, however, to realize that the *whole set* (i) is always at issue, logically, if 1, 2, and 3 are grasped. Moreover, explanations given to factors A, B, and C must be such that they make sense for variables which involve their mixtures, *and for those, also, which do not involve the factors at all,* namely, of class 8. These matters are of particular importance in Q-technique and dependency factor analysis, as we shall see in due course. The classes of set (i) are the basis, in Q, for classifying variates into "pure" or "mixed" *types.*

Precisely the same set of possibilities occurs for the variance analysis of a sample. For the case of three main effects, X, Y, and Z, and assuming no significant interactions, any Q-sort has open to it the set of eight possibilities (i) above—X alone might be significant, as shown by F-tests, or Y alone, or Z alone, and so forth.

We propose to call the set of possibilities (i) a case of "simplest structure" for orthogonal factors, with the object of drawing attention to *all* the possibilities, i.e., for the "mixed" as well as the "pure" cases and for the *null* case as well as the "pure." In dependency factor analysis, therefore, the essential object is always to achieve *simplest structure* for all the possible arrangements of any given set of factors.

This, again, may seem to be a very innocent step to take. Yet it has two important logical consequences. The factorist has two possibilities open to him as he pursues his analysis. For a given number of variables he can look for a solution involving as many "pure" classes as possible, as has happened in Thurstone's pursuit of simple structure. Or he may look for only a few factors, which, *together with their combinations*, cover the same data. In the latter case he expects only a few "pure" cases. The latter, it seems to us, may well be the more parsimonious. From the standpoint of dependency factor analysis and of experimental method in general, the interest of the investigator is in main effects. Naturally, almost any data are capable of being analyzed almost indefinitely into smaller and smaller parts, and all sorts of effects are likely to be about, in a factor study, in which we need not be unduly interested.

Moreover, one of the most important features of *simplest structure* is the possibility that explanations we may offer to "pure" factors may or may not offer an explanation to variates involving two or more, or *no*, factors. If main effects or factors have true explanatory power, they should provide pertinent explanations for the "mixed" cases. This could be the acid test, indeed, of a dependency factor analysis.

THE EXPERIMENTAL SITUATION

The experiments we undertake in psychology, using the principles under discussion, are more elaborate in their rationale than those worked at in practice by either agriculturalists, on the one hand, who have made most use of Fisher's methods, or psychometrists, on the other, who have made much use of factor analysis in R-methodology. The reader is now able, perhaps, to take stock of the somewhat topsy-turvy scheme of things with which Q is concerned. Fisher's methodology is primarily a matter of experimental science, for independent and dependent effects. We are about to use these

methods, in every detail, except that only our explanations are at issue and usually not the propositions with which we are experimentally involved. The latter are essentially matters for factor analysis, which, traditionally, is not an experimental method. Nevertheless, all the usual statistical procedures of both variance and factor analysis are employed, except that we give priority, theoretically, to Fisher's concrete estimates of experimental errors rather than to large-sampling doctrine.

When Fisher experiments with a balanced design or with any others of the kind, he is concerned, usually, with only *one* dependent variable. The design may represent plants in some order of treatment, and the dependent variable may be the weight of seed produced by each. The independent variables, where significant, are

TABLE 6

Independencies	Levels			No.	D.F.
A, sunlight	(*a*) Much	(*b*) Little	(*c*) None	3	2
B, chemicals	(*d*) C_1	(*e*) C_2	(*f*) C_3	3	2

assumed as the "causes" of the results—thus sunlight and some particular chemical treatments may be the independent variables, as in Table 6. In balanced design there are $3 \times 3 = 9$ cells for this design, namely:

a a a	*b b b*	*c c c*
d e f	*d e f*	*d e f*

The experiment, let us suppose, concerns the effect of these main variables *A* and *B* upon the weight of seed produced by a particular variety of plant. The plants, one to a pot, are grown up to a point under uniform conditions. We decide upon, say 10 replications, so that 90 plants will be required to cover the design in balanced design. The pots are first placed randomly in the 9 cells or sets of conditions, until each has 10 pots. The independent variables are then brought to bear upon the cells—the pots for *a d* will receive a lot of sun and be fed on Chemical C_1 . . . , and so on. At the conclusion of seeding, the seed produced by each plant is weighed, and this constitutes the dependent variable, ready for analysis by division of its variance (Table 7). The analysis depends fundamentally upon the premise

that the replication variances, for the 9 cells, each for 9 degrees of freedom, will be homogeneous, i.e., such as error variances alone would provide for each, and for all taken together (for the 81 degrees of freedom). Relative to this estimate of error variance, the main effects and the interaction are tested for significance by F-test procedure.

It will be noticed that the principle of randomization is doubly represented. The plants, homogeneous as far as one can guarantee it, are placed in the cells so that chance alone enters into the decision—we do not pick plants which "look good" first and place them in favored cells. And when any set for a cell is subjected to the experimental treatments, it is supposed that nothing but chance factors will distinguish between them, if the independent variables are

TABLE 7

		D.F.
ΣA		2
ΣB		2
$\Sigma A \times B$		4
Σ Replication	$9(10-1)=$	81
Total		89

without effect. The first step is merely an attempt to guarantee the latter conditions—we might most sensibly call the latter the "principle of experimental error." For the sake of clarity, therefore, we shall reserve the term "randomization" for the first condition and "experimental error" for the second. The latter, rather than the chance placing of plants in cells, is the basic experimental principle or condition.

We ask, then, in what respect such designs as we employ in Q-methodology, e.g., as for Jung's theory (chap. iv, Table 2), are comparable to Fisher's experimental procedures. Consider Table 2 of chapter iv. It is a $2 \times 2 \times 4 = 16$ "cell" design, balanced for three main effects or independencies. Eighty statements are allotted these cells—*but not at random*—for 5 replications of the design. If the 80 are used for a Q-sort, the variance is divisible into the components in Table 8. The analysis depends upon the test for homogeneity of the replication variances, which define the experimental conditions. That is, each cell represents a little experiment, repeated Z times. If the *principle of experimental error* can be supposed to mediate for each

such cell, the fundamental experimental premise is fulfilled. The main effects and interactions are thereupon tested against error variance, using the familiar F-test procedures and small-sample doctrine.

It is a mistake, then, all too easy to make, to suppose that the *statements* of a Q-sample correspond to the plants of Fisher's case. As we saw, the statements are certainly not placed in the various cells of a design according to chance principles. *What is at issue, instead of plants, is an operator's acts of judgment, or decisions, made about the statements. These acts should be made randomly.*

In experimenting, the operator who provides a Q-sort is not aware of the principles upon which the Q-sample has been constructed. He

TABLE 8

	D.F.
ΣX	1
ΣY	1
ΣZ	3
ΣXY	1
ΣXZ	3
ΣYZ	3
ΣXYZ	3
Σ Replication	$16(5-1)=64$
Total	79

is not aware that the main effects or independent variables are involved. All that confronts him are the statements of the sample, thoroughly shuffled.[2] With these he performs a Q-sort under specified conditions of instruction. The Q-sort is the dependent variable. The statements of the sample gain such-and-such "significance," we suppose, owing to the action of the specified processes (represented in the design by the independent variables, or independencies, as we are more likely to call them). Thus, for a statement in a cell *a c e* of Table 2 of chapter iv, the expectancy is that processes of *introversion*, *conscious mechanisms*, and *thinking function* may mediate. If these are genuine effects, then they should be testable. If they are not, then the *principle of experimental error* should prevail.

Thus we might say that we are working with "as if" conditions. If

2. A procedure, incidentally, that corresponds in purpose to the *principle of randomization* as more narrowly defined above.

we suppose that there are certain independent variables (such as we represent in our designs), these could explain the conditions which make X's personality what it is. All is hypothetical. But we can test the consequences of it all, which are that the *principle of experimental error* will mediate at the replication variances and that significant effects may be observed for the main effects. These are testable issues. It is a matter of *fact* whether the principle of experimental error is acceptable and whether significant effects are observed.

It is clear, therefore, that we do not have our independent variables under the same kind of control as occurs in a Fisher study—we cannot ladle out a "systematized anxiety" or a "thinking function" as we can a chemical substance. But the actual effects, even for a chemical product, are "theoretical" rather than concrete substance. We retain this "theoretical" aspect in Q-study by giving the reminder that the balanced or other designs primarily concern such theoretical explanations. Our testable propositions concern the different variates which produce the Q-sorts; those for the *design* itself are of purely *explanatory* significance, as we have said several times.

Some different principles are also brought into the picture by *factor* analysis. In variance studies the explanation of facts is dependent upon the specified independent variables (such as A and B of the plant study, or X, Y, and Z of the Jungian), or upon their *interactions*. If, in the plant case, only chemical C_2 and much sunlight give significant results (there being no significant interaction), the inquiring mind will explain the facts accordingly. This means merely that he now knows along what lines his further inquiries are likely to be fruitful, and along which, not. He can forget about treatments C_1 and C_3 and begin to wonder what there is about C_2 that gives the significant results. In our studies, however, we may wish to replace the independent variables X, Y, and Z by others, pointing, that is, to different explanations. This would have to be done, in Fisher methodology, by designing a new experiment for the new independent variables—say, for chemicals C_4, C_5, C_6, all derivatives of C_2. We achieve something of the kind in our Q-studies by rotating centroid factors to different solutions.

CHAPTER VII

SOME EXAMPLES

INTRODUCTION

THE principles and issues discussed in the foregoing chapters have been abstract and theoretical. It will be helpful, now, to look in detail at a few practical examples involving them. We choose for the purpose an illustration for singular propositions and variance analysis, another for the treatment of a problem in Q-technique terms, and a third for the comparison of factor and variance procedures. With these in mind, it should be easier, thereafter, to turn to the wider applications of the postulatory-dependency method, as we call Q in its more general terms.

EXAMPLES FOR A SINGULAR PROPOSITION

Theories, we have said, should be regarded as growing points for *singular* propositions (92, 166). Many attempts have been made in the past to measure introversion-extroversion on the supposition that *all persons are introverts or extroverts in some degree habitually*. The proposition is typical of all R-method studies, in which everyone is supposed to have every attribute in some degree. This seems self-evident. However, such general implications are nowhere necessarily involved in Jung's theory. The concern, instead, can be with singular propositions of the following kind:

> Proposition 1: This particular person X is either introvertive or extrovertive habitually, or neither.

This we can at once test along Q-lines in terms of the *theory* of introversion-extroversion, without reference to any person other than X, without the use of any norms or standard scales, and without operational reference to any individual differences (that is, strictly, without involving individual differences as a *modus operandi* for the transitory postulate; see p. 58). We merely invite X to offer, say, a self-description, as he conceives himself to be habitually, in terms of a Q-sort, for a *structured* sample of Jungian statements, such as is

provided in chapter iv. Reference should be made to the structure of Table 2, chapter iv.

In a particular case, for example, we had available a sample of 160 statements, similar to those of the appendix to chapter iv, but for 10 replications of the design. We invited our landlady at the time to

TABLE 1

	Most Characteristic of Me Habitually								Least Characteristic of Me Habitually	
Score	8	7	6	5	4	3	2	1	0	
Frequency	8	12	20	24	32	24	20	12	8	($n=160$)

TABLE 2

	"Cells"			
	a a a a / *c c c c* / *e f g h*	*a a a a* / *d d d d* / *e f g h*	*b b b b* / *c c c c* / *e f g h*	*b b b b* / *d d d d* / *e f g h*
Scores	0 5 1 6	5 4 5 4	2 6 5 6	7 8 6 4
	6 4 3 4	4 4 4 3	8 5 4 6	6 7 5 6
	6 3 2 3	3 2 3 2	7 3 4 4	5 4 4 6
	5 2 0 0	2 0 1 4	6 2 3 3	4 4 3 6
	4 2 0 0	7 1 8 1	4 8 2 8	3 2 5 3
	7 6 1 6	4 4 3 6	3 6 2 7	2 7 3 3
	4 4 4 5	1 3 0 5	5 5 7 5	2 6 5 6
	4 3 4 1	1 3 4 5	3 4 1 5	5 3 2 4
	3 5 4 8	5 1 7 1	5 7 4 2	2 2 8 5
	2 0 1 5	8 2 2 4	4 3 6 6	7 5 6 7

offer a self-description for the frequency distribution of scores[1] given in Table 1. The actual scores provided in this way in the respective "cells" of the design are shown in Table 2. The analysis in

1. The statements are typed on cards, one statement to a card, and the subject first reads them through, in order to grasp their import. They are then shuffled, and she proceeds to the Q-sort. For practical hints on how to conduct a Q-sort see pp. 60 and 231. Usually the cards are first divided roughly into three piles by the subject, one for those that characterize her positively, one for those that do not do so, with the doubtful or neutral ones in between. The piles are then teased apart until the required frequency is reached. For ease of application we rarely employ as many as 160 for a single Q-sort. In the present case the 160 were divided into two samples of 80 each, the Q-sorting being repeated for each in turn and the results then combined.

terms of "expectancies" proceeds as in Table 3. The F-test shows that only the first of these sums of squares is significant. The data then clearly indicate that it is b, i.e., the introversion level, which has been given the significantly large score. That is, our landlady operates in such a way as to suggest that, in terms of Jung's theory, she is introvertive. None of the other effects is significant in her case.

The replication variances are tested for homogeneity in the usual way. If the test proves satisfactory, we may suppose that the experimental subject has operated with the statements according to the principle of randomization (Fisher, 68), so that contingencies of any obvious kind are not at issue. Clearly, anyone can take part in

TABLE 3

	Source of Variance	Sums of Squares	D.F.	"Expectancy"	F
W	Between attitudes	70.22	1	70.22	18.14
X	Between mechanisms	0.22	1	0.22	0.05
Y	Between functions	15.35	3	5.12	1.32
	Interactions	36.61	10	3.66	0.94
	Replication	557.50	144	3.87	....
Total	....	679.90	....	....	....

such an experiment, and the conditions can be made as specific as we care to make them: Mrs. X can describe herself (a) as she was ten years ago, (b) at her happiest, (c) as she feels herself to be at a party, and so on. Every Q-sort, for anyone, can be analyzed in the above manner, prior to any factor analysis of a number of such variates (whether for different persons or for one and the same person). Facts can be accumulated in this manner which Jung would presumably explain by his theory. As we said earlier, it is not supposed, of course, that any theory is proved or disproved by a few singular studies of the above kind. Nor, if thousands of such singular propositions are tested satisfactorily about a theory, is the theory necessarily acceptable. But valuable theories point to interesting propositions, and we shall examine Jung's theory from this standpoint later, to show in more detail how it could have been put to test, but never was, because R-factorists thought that they were being scientific when they worked with general propositions.

STUDY OF THE "SELECTIVE SERVICE DILEMMA"

THEORETICAL FORMULATIONS

We choose as our next example a study of the attitudes of some students about *selective service* in the armed forces. At the time the study was made (June, 1951) the press and radio were debating this matter, and many college students, of course, were deeply involved in the issue. The problem was to reduce a student's *thinking* about selective service to operations, so that we might experiment upon it. This is achieved along Q-lines as follows:

In order to make a beginning, a collection of many statements was made of opinions and attitudes that students had, at the time, about selective service. Some were taken from letters appearing in the press, and others from a test of a projective kind, namely, an *editorial test.*

EDITORIAL TEST

Editors of newspapers, probably, are free to express their views about current events, or about the everyday problems of life. Suppose that an editor has decided to write a "leader" about Selective Service, and that his opening sentences are as given below. If you were in his position, what would be the finished editorial?

Try to put yourself in the editor's shoes. Express yourself freely. Don't bother much about grammar or style. Just write a few more paragraphs as quickly as you can as a continuation of the following opening sentences:

"SELECTIVE SERVICE DILEMMA"

Every young man, and woman, has to face it. Yet little or no helpful information is available for those who will soon be face-to-face with Selective Service, or who are enrolled as Reservists in one of the Services. Nobody knows much about it. And what they do know, or think, cannot be made public. More than ever before. . . .

(Write two or three paragraphs to continue this opening.)

From the responses given to this by some 30 students implicated in selective service, a large number of statements could be culled. From this and the other sources a list of 200–300 was put together. The following are examples:

1. The young man must wave his flag, but with a firm purpose.
2. What we need is a brief booklet of essential information about it.
3. The dilemma cannot be completely solved: we are living in troubled times and must learn to accept crises with some equanimity.

4. It is unfair and cruel to young men.
5. Patriotism is not a sufficient excuse for war and excess.
6. Rumor follows rumor—all is a vast tissue of irresponsibility and muddle.
7. The draftee must determine whether to enlist now of his own free will, or wait for the draft and all its restrictions.
8. The individual does not count—more serious matters lie ahead.
9. The newly acquired security of thousands of young couples is about to be destroyed.

A perusal of the collection suggested that many students had adopted a more or less reasonable attitude about selective service, whereas others were clearly emotionally involved, if not upset. The former, at least overtly, were socially conforming, as statement No. 2 above might suggest. The emotionally toned statements betrayed some disquiet along two lines: the student either made highly *personal* reference to himself (e.g., No. 4, if the statement has the implication ". . . it is unfair and cruel to young men *like me*"), or else made reference in an emotional manner to the policies, etc., of outer agencies (e.g., No. 5 or No. 6).

TABLE 4

Independencies	Levels	No.	D.F.
A, attitude	(*a*) Reasonable (*b*) Emotional (*c*) Doubtful	3	2
B, orientation	(*d*) "Personal" (*e*) "Impersonal" (*f*) Doubtful	3	2

Conclusions of this kind can be represented formally as "effects" in a balanced block factorial design, namely, for the present case, as in Table 4. Each of the two main effects, *A* and *B*, is represented at three levels, a place being retained for "doubtful" statements, that is, for any that we cannot agree to place in the other levels. This little design, then, represents our "theory" in a formal manner.

There are 9 combinations of these levels of Table 4, one effect at a time, namely:

$$\begin{matrix} a\,a\,a & b\,b\,b & c\,c\,c \\ d\,e\,f & d\,e\,f & d\,e\,f \end{matrix}$$

The basis exists, then, for composing any number of structured samples to fit the design, for any number of replications. Usually, a

sample of size about 80 is desirable, and therefore we replicated on the design 8 times, making $n = 72$.[2] For the sample used below, the statements were placed in the respective "cells" of the design by Miss C. Kris.[3]

Given a structured sample of this kind, it is possible to conduct innumerable experiments upon, say, *any* student or students implicated in selective service. They can be got to "think" about selective service under various conditions of experiment or instruction and to represent the outcome by a Q-sort in the familiar Q-technique fashion.[4] Thus we might ask a student to array the 72 statements from those he thinks represent his opinion *most*, to those *least*. Or, given the same instructions as for the "selective service dilemma," he can be called upon to complete the editorial as a Q-sort, placing statements at the "most significant" end of the distribution which he

TABLE 5

	D.F.
ΣA	2
ΣB	2
$\Sigma A \times B$	4
Σ Replication	63
Total	71

would emphasize most in the editorial, and those "least" at the other end. The scores he so provides can be analyzed for him, for the usual division of the variance, as shown in Table 5. The replication variances can be tested for homogeneity (33), and the familiar F-test applied to see whether effect A or B or both are significant. If interaction is significant, then an effect is at issue other than A and B, i.e., something not anticipated, but "found."

If A is significant we shall know what has made it so, namely,

2. A copy of the sample is in Appendix I to this chapter.

3. We wish to thank Miss Kris for conducting the experiment for us.

4. That is, a score is given to each of the 72 items of the sample, on the basis of a forced quasi-normal frequency distribution. For the present example we used the following distribution:

	Most "Significant"										Least "Significant"	
Score	10	9	8	7	6	5	4	3	2	1	0	
Frequency	2	3	5	8	12	12	12	8	5	3	2	(n=72)

whether $a > b$, or $b > a$, or whether c is involved. Similarly for B. The analysis can be undertaken for every Q-sort made either by one subject under different conditions or by several subjects under the same or different conditions of instruction. However, when many arrays are at issue, it is more economical of effort to correlate and to factor them. The variance analysis can then be carried out on *factor-arrays*, each of which, of course, merely subsumes one or more of the original Q-sortings.

A TYPICAL STUDY

As an example, 18 students were taken, all implicated in selective service. Each looked at the instructions for the *editorial* test (see above), but, instead of writing a continuation of the editorial, each represented his thinking about the dilemma by a Q-sort, using the structured sample ($n = 72$).[5]

Table 6 gives the correlation table for the 18 Q-sorts. Two centroid factors subsume the data, which requires little, if any, rotation. It is clear that persons Nos. 2, 6, 7, 8, 11, 13, 14, 15, 16, and 17 cluster roughly together, comprising the positive end of factor I and that, orthogonal to it, factor II has Nos. 1, 3, and 10 at the positive end and Nos. 4, 5, and 9 at the negative. Perhaps No. 18 inclines to be negative for I and positive for II.

The factor-arrays are next calculated. This is an important procedure in Q and is outlined in greater detail later, in chapter viii (p. 174); reference to the matter was also made in chapter v (p. 73). It consists of estimating the *factor scores* for each of the statements of a sample, in standard terms. Thus there may be, say, ten variates, all differently loaded in a factor. The scores gained by a *statement*, ten in all (one with respect to each variate), are added, giving greatest weight[6] to those scores belonging to the variates with the largest loadings. For factor I, in the above case, scores for the variates Nos. 2, 6, 7, 8, 11, 13, 14, 15, 16, and 17 are added. For factor II they

5. The statements, each on a 4 × 2-inch card, are first well shuffled, and, of course, the subjects are quite unaware of the structure.

6. If f_a and f_b are the factor loadings for factor f in two variates a and b, respectively, the "weights" for the scores gained by any statement are in the following proportion (148):

$$\frac{\omega_b}{\omega_a} = \frac{f_b\,(1 - f_a^2)}{f_a\,(1 - f_b^2)}\,.$$

TABLE 6*

CORRELATION COEFFICIENTS FOR 18 SUBJECTS
(n = 72 Statements)

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	Centroid Factors I	Centroid Factors II	Rotated Factors I′	Rotated Factors II′
1	—																		30	47	24	50
2	16	—																	57	06	54	14
3	−23	05	—																−03	50	−09	49
4	−07	25	22	—															13	−31	16	−29
5	−16	17	20	19	—														30	−44	34	−39
6	25	48	−02	−01	23	—													58	03	55	10
7	28	25	−01	15	18	24	—												55	−09	55	−03
8	34	32	−02	05	22	44	50	—											71	−04	71	03
9	−18	−07	33	24	19	05	01	22	—										08	−41	11	−40
10	−19	−10	23	14	12	−21	−03	−07	30	—									−08	50	−13	48
11	04	42	−08	01	22	48	43	56	14	−10	—								70	−07	69	−01
12	16	16	11	16	14	14	06	12	02	−11	16	—							28	01	26	04
13	30	29	−31	−10	17	40	42	60	07	−17	51	26	—						75	17	72	25
14	12	36	..	14	40	38	34	49	15	13	54	17	43	—					71	−25	72	−17
15	10	30	12	20	13	21	33	18	12	06	30	25	36	38	—				47	−12	43	−06
16	05	22	06	12	49	44	38	47	20	04	48	01	45	57	23	—			62	−34	63	−25
17	23	44	−06	02	08	21	22	45	28	−00	35	06	42	48	28	41	—		61	02	59	09
18	−20	−31	35	−01	11	−06	−13	−02	20	20	−08	−23	−23	−18	−25	−03	−38	—	−31	39	−34	34

* Decimal points are omitted.

are for Nos. 1, 3, 10 (positively) and Nos. 4, 5, 9 (with scores *reversed*, because of the negative loadings). The statements are then ranked in order of their factor scores, for each factor in turn. Thus the factor-array for factor I consists of a Q-sort that a person X could have given, in fact, with the same result as all 10 persons, Nos. 2, 6, 7, 8, 11, 13, 14, 15, 16, and 17, give in the aggregate or "on the average"—the average being a weighted one. It has the property that each of these ten variates will correlate with it by an amount equal to its factor loading with the factor (148).

Thus, when the 72 statements are ranked with respect to factor scores for factor I, a statement will be at the top of the rank with a score of approximately +3.0 in standard terms; at the other extreme there will be a statement whose score will be of the order of −3.0. The statements in the middle of the array will have score 0.0; and the standard deviation of the scores will be 1.0. For factor I this rank order or factor-array begins as follows:

Factor Scores
(Standard Scores)

+3.1 No. 9: The channels of public communication, radio, cinema, newspapers, and television should be used to disseminate information concerning selective service issues.

+2.9 No. 2: The best thing for me to do is to obtain as much information as is possible about what I must expect.

+2.9 No. 13: What information there is must be made freely available, in a form that everyone can understand.

These are the statements given highest "significance" by the students loaded in factor I, and obviously they indicate reasonableness and good sense.

Factor II, on the other hand, as clearly represents a certain emotionality. The most "significant" statements for it, at its *positive* end, are as follows:

Factor Scores
(Standard Scores)

+2.8 No. 47: Why does such a state of affairs have to exist; it makes life seem futile.

+2.6 No. 34: Why don't the "Big Wigs" get together to avoid such a thing as selective service? Does a wife bear children to bring them up fatherless?

+2.6 No. 25: You can't blame me for wanting to know if the cause is worthy of laying down my life!

+2.2 No. 32: How can they blame me for panicking about planning my future, since they give me no basis upon which I can plan?

The negative end of the same factor, i.e., statements placed highest by persons Nos. 4, 5, and 9, has the following statements most "significant":

Factor Scores
(Standard Scores)

−2.7 No. 45: By following the way of hate, man has come to this mess.

−2.7 No. 40: It scares me to think that we slough off the basic questions asked by our youth with vague generalizations or propaganda slogans. We must face the issues squarely, even if our answer is as yet unclear.

−2.3 No. 22: There is a chasm between the government and the people and within the people itself; this must be bridged, but how?

−2.0 No. 5: People are frightened; they try to make decisions too rapidly. The best thing that I can do is to be as patient as possible and see how things gradually develop.

Clearly the *positive* end has a personal, inward-turning reflectiveness, whereas the *negative* is directed upon "man," "propaganda," "the government," and "people."

The factor analysis thus supports the original hypothesis: we have merely "interpreted" the factors by looking down their ranked arrays, and the statements originally earmarked as "reasonable" appear at the end of factor I, whereas the emotionally toned ones appear at the extremes of factor II. The latter is bipolar. The former is not, for the present data. For, since factor II represents a certain emotionality orthogonal to I, the *negative* end of factor I would be still more strongly emotional: that is, downright fear, anxiety, and anger or the like would mediate, and not the more controlled emotionality which runs all along factor II. Figure 1 indicates the matters at issue.

It is merely incidental that the centroid factors need little, if any, rotation to provide these results. In more complicated data, resort has to be had to rotations.

We may now examine the arrays for the 18 men separately, by way of variance analysis. It is clear that doing so would merely give us a basis for classifying the men into groups of those alike with respect to their significant *effects* and the order of significance of their *levels*. For the present it is sufficient to conduct the variance analysis

on the factor-arrays, for factor I and for factor II, taking the positive and negative ends separately in the latter case. For this purpose the weighted standard *factor scores* were transformed to give the forced frequency distribution used for the initial Q-sortings. The total variance is then the same for each factor.

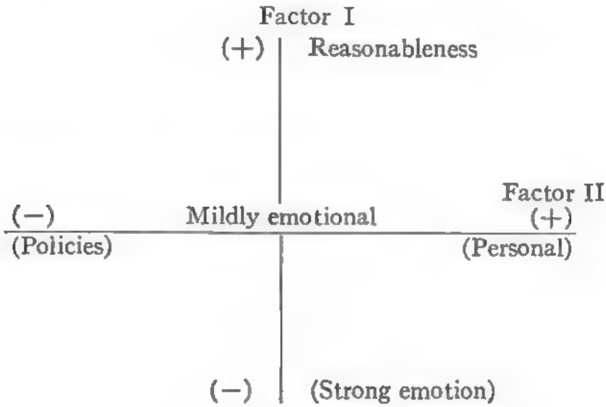

FIG. 1

TABLE 7

	D.F.	Sums of Squares	Mean Square	"Expectancy"
ΣA	2	232	116.00	71.16*
ΣB	2	15	7.50	4.60
$\Sigma A \times B$	4	24	6.00	3.67
Σ Replication	63	103	1.63	....
Total	71	374	....	....

* These data are significant, as shown by F-test, at the smaller than 1 per cent level.

The analysis for factor I is given in Table 7. Only ΣA is significant by F-test, which is greater than the *4.98* required at the 1 per cent level for 2 and 63 degrees of freedom. It is next found that this is attributable to level a (Table 4), i.e., $a > b = c$. Normally we would test the replication variances for homogeneity (33); we do not do so here because averaged, and not operationally defined, error estimates are involved.

The analysis for factor II (positive end) is given in Table 8. Here both ΣA and ΣB are significant at less than the 1 per cent level, each

for 2 and 63 degrees of freedom. Examination of the respective levels shows that $b > c > a$ in the one case (Table 4) and that $d > f > e$ for effect B. That is, the array is significantly *emotional* and is *personally* orientated.

For the negative end of the factor, i.e., using the summed arrays for students Nos. 4, 5, and 9, the analysis is shown in Table 9. The same effects are significant. In case A the significance is due to the

TABLE 8

	D.F.	Sums of Squares	Mean Square	"Expectancy"
ΣA	2	107	53.50	28.61*
ΣB	2	122	61.00	32.62*
$\Sigma A \times B$	4	27	6.75	3.60
Σ Replication	63	118	1.87	....
Total	71	374	....	....

* These data are significant, as shown by F-test, at the smaller than 1 per cent level.

TABLE 9

	D.F.	Sums of Squares	Mean Square	"Expectancy"
A	2	124	62.00	29.66*
B	2	97	48.50	23.20*
$A \times B$	4	21	5.25	2.51
Replication	63	132	2.09	....
Total	71	374	....	....

* These data are significant, as shown by F-test, at the smaller than 1 per cent level.

levels being different in the order $b > c > a$, and for B, $e > f > d$. That is, the array is significantly *emotional*, but the *impersonal* level is uppermost. The original hypotheses are therefore supported.

DISCUSSION

The example illustrates a number of methodological matters which now distinguish Q-technique.

First, the "subjective" interpretation of a factor is replaced by its orderly examination in terms of the postulated "theory" in the *sample*. The customary explanation by inspection of the factor-

arrays gives way to their variance analysis. In principle, both the *dependent* and the freely interpretative procedures are permissible, although in comparable studies in agronomy Fisher (68) would argue that one should play the dependent form of analysis exclusively, so as to save one's self from a posteriori reasoning and, perhaps, overmuch *hindsight.*

Second, the "theory" is made explicit in a balanced block design: it is surprising how far-reaching this simple bit of logic can be.

Third, the earlier methodology of factor analysis, which was *inductive* in form, for *interdependency* analysis (93), here gives way to a frankly theoretical and experimental approach. The usual response of quite sophisticated factorists, when they first meet with an example of the above kind, is to resist it; for, they say, does it not merely consist of getting out what one puts in to start with? The question perhaps means, as well, that nothing very profound, important, or "fundamental" is at issue. Our reply is that very profound matters can indeed be dealt with: and, as to the former question, one, of course, *anticipates* certain results, as in science generally. But the operations are always those of *subjects*, and these are *facts*. Interaction may occur, suggesting that effects other than the specified ones are involved. Moreover, for even a simple design like that of Table 4 the effects can be significant in many different ways, for different arrangements of the levels. Thus for effect A of the table there are possibilities as follows:

$a > b > c$	$a > c > b$	$b > a > c$
$a > b = c$	$a > c = b$	$b > a = c$
$a = b > c$	$a = c > b$	$b = a > c$
		and so on.

If there are p such possibilities for one *effect*, q for another, and r for another, there are $p \times q \times r$ such possibilities in all. Finding out *which* are significant may be the whole point of an inquiry, or we may be able to predict which of many, in fact, will prove significant. Again, although in terms of main effects we may know what to expect empirically, the operations alone can tell us which Q-sorts will be significant for which effects. Thus only the Q-sorts tell us which students are of factor-type I, and which of factor-type II (+) or (−) in the above study.

It will be noticed that, in principle, we leave "cells" open for

levels of unspecified kind (the "doubtfuls"), partly so as to be sure that the other levels may represent genuine components.

It might be objected that this is too simple—after all, could we not question the students and find these answers without the fuss of factor analysis and variance design? Clearly, the reduction of a student's "thinking" to a simple set of operations is a scientific objective, and, like all sound procedures of the kind, the achievement rapidly proliferates and suggests new possibilities of experimenting. We may take it for granted that, no matter how many students are tested in the above way, facts of the kind will be provided. Certainly we would *not* be interested in questions of this kind: What proportion of the population of all students are of factor-type I? and What proportion of factor-type II? Instead, we would be more interested to pursue a properly experimental and analytical methodology. Thus we might choose *one* representative case of each type, one, *X*, for factor I, and one each, *Y* and *Z*, for the positive and negative ends of factor II. Each would then be made the object of an intensive investigation along theoretical lines, i.e., the psychological thinking would *precede* the statistical analyses and not vice versa. Thus, about *X* we might wonder whether, as the psychoanalyst supposes, the overt reasonableness and social conformity is not a complicated *rationalization* rather than a sublimation. We can now "get at" questions of this kind quite easily. In the present case, for example, we would take *X* and compose a structured sample from statements that he himself makes about selective service, with the hypothesis of rationalization and sublimation represented as one effect, along with other effects as the data suggest. Then *X* could provide several Q-sorts, under different conditions of instruction, all designed to determine under what conditions a breakdown into rationalized behavior will occur. The experimentation along these lines is almost unlimited. Similarly for *Y* and *Z:* in the one case a somewhat hysterical "character" formation may be indicated, and in the other a compulsive or obsessional form. Each again would be readily open to our procedures, usually with respect to samples of the subject's own statements, whether consciously given (as in interviews), or *un*consciously (as in his projections about *others* in a TAT test). Thus we never regard any study, such as the above about the 18 students, as anything other than a link in a long theoretical

chain. The outlook is one of experimenting with a psychological theory behind one, and not that of trying to discover "unitary" or "primary" factors and the like. Always *singular* propositions are at issue and never any "general implications," operationally defined.

EXAMPLE FROM EXPERIMENTAL AESTHETICS

Our third detailed example is from experimental aesthetics. It is in part a reminder that Q-method covers more than verbal statements. In the present example a structured sample of colored designs is used, constructed especially to put certain aesthetic principles to an empirical test.

The problem is as follows: Cézanne, the great French painter, developed a certain freedom in the use of "form" (10). This, it seemed to us, corresponds to freedom from the restrictive effects of "shape dominance," to which, in a very general context, the Gestalt psychologists have drawn attention. Thus Kohler (98) remarks that a *square* is regularly drawn "on its bottom," i.e., □ and not ◇. An aesthetic principle may be hypothesized, therefore, to the effect that freedom from "shape dominance" will often be indicative of a developed aesthetic sensibility. A second principle also interested us, to the effect that aesthetically pleasing art involves *concentration of effort:* a design that can be grasped easily as a whole, whatever its complexity, will be more pleasing than one that calls for dispersion of attention because it is not easy to grasp it as a whole. The principle has many different names in aesthetic theory (30). The problem is to put these two principles to an empirical test. One way to do it is to represent the two principles in balanced Fisherian design for material from an *art-form* test which we have described elsewhere (169).

APPROACH TO THE PROBLEM

In this *art-form* test artistic designs are made out of five standard pieces of colored paper, which are pasted upon a white background sheet. The following are the five pieces:

	Inches
Square:	2×2
Square:	$1\frac{1}{2} \times 1\frac{1}{2}$
Square:	1×1
Rectangle:	2×1
Rectangle:	$1\frac{1}{2} \times 1$

The shapes can be of different colors. It is possible to make artistic compositions with these papers according to a combination of two main principles: the shapes may be used in *regular* or *irregular* positions relative to the "frame" of the background; and they may *overlap* or not. The two may be represented as two main effects in a balanced factorial design (Table 10). There are four combinations of the main principles X and Y:

$$\begin{array}{cc} a\,a & b\,b \\ c\,d & c\,d \end{array}$$

Figure 2 provides an example of each of these combinations. It is possible to make, or to collect, designs to fit these conditions for any number of replications. The possibilities of testing the hypothesis of *shape dominance* in relation to aesthetic sensibility lie in effect X, and for *concentration of effort* in effect Y.

TABLE 10

Independencies	Levels		No.	D.F.
X, shape dominance.....	(*a*) Regular	(*b*) Irregular	2	1
Y, shape concentration..	(*c*) Not overlapping	(*d*) Overlapping	2	1

In passing, this representation of X and Y is another illustration of our thesis that principles, arguments, conclusions, and theory, over a very wide range of concern in psychology, can be dealt with in this formal manner. But we can also add additional effects, either as "controls" or in order to examine the experimental variables in a variety of different contexts. Thus *color* (Z) is important in aesthetics: we are not interested in this study to inquire into color as a significant effect, but it is almost essential to use color so that the shapes will not be confused with one another in finished compositions and so that the compositions will be aesthetically more pleasing. It is not difficult, however, to include *color* as an effect which can rarely be significant, e.g., by making all the compositions homogeneous for color.

Thus we add color (Z) as an effect, but steps will be taken to reduce to a minimum the possibility of a significant effect for Z. The complete factorial design is shown in Table 11. The levels A, B, C, D, and E are for different arrangements of five basic *colors*, red (R),

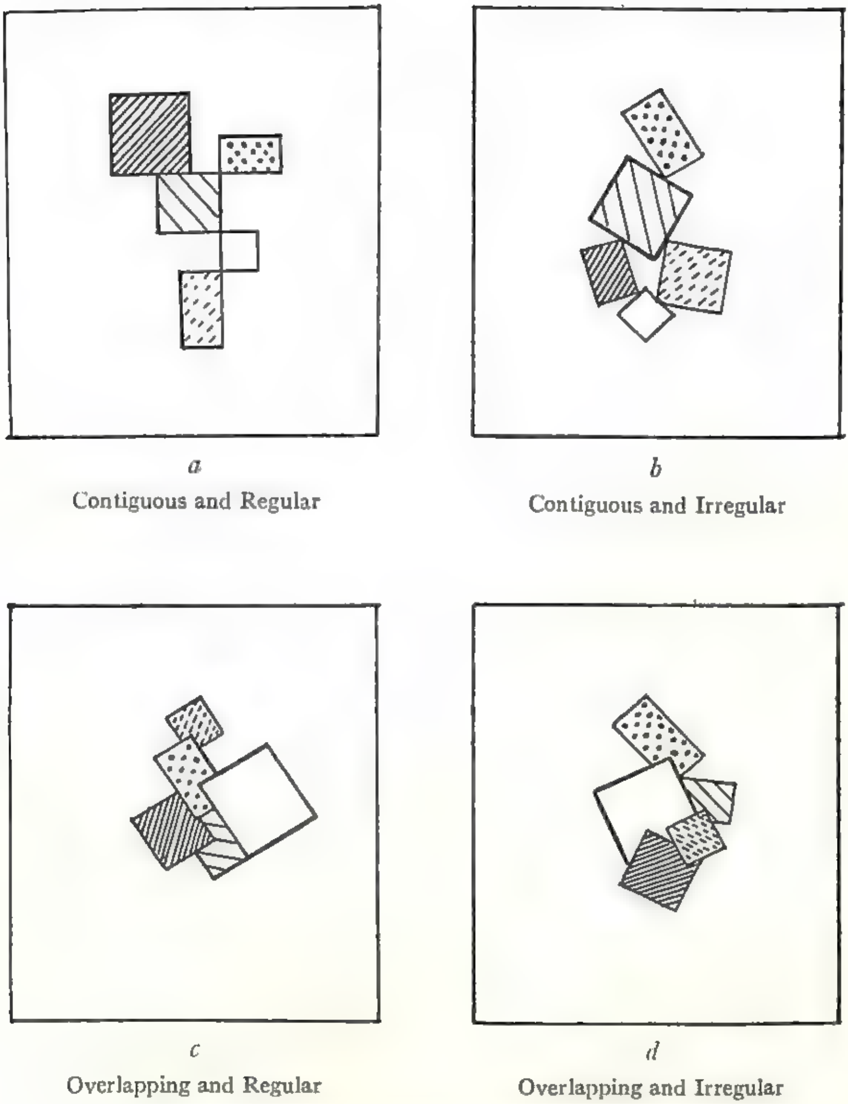

FIG. 2.—Different-colored squares are represented

green (G), brown (B), yellow (Y), and maroon (M) (Table 12). That is, *level A* consists of compositions using a 2×2 red square, a $1\frac{1}{2} \times 1\frac{1}{2}$ green square, a 1×1 brown square, a 2×1 yellow rectangle, and a $1\frac{1}{2} \times 1$ maroon-colored rectangle.

The combinations for a balanced design for Table 11 are as follows:

$$\begin{array}{ccccc} a\ a\ a\ a\ a & a\ a\ a\ a\ a & b\ b\ b\ b\ b & b\ b\ b\ b\ b \\ c\ c\ c\ c\ c & d\ d\ d\ d\ d & c\ c\ c\ c\ c & d\ d\ d\ d\ d \\ A\ B\ C\ D\ E & A\ B\ C\ D\ E & A\ B\ C\ D\ E & A\ B\ C\ D\ E \end{array}$$

For our experimental purposes a structured sample was composed for Table 11, for 6 replications, making $n = 120$. That is, 6 composi-

TABLE 11

Independencies	Levels					No.	D.F.
X, shape dominance	(*a*) Regular			(*b*) Irregular		2	1
Y, shape concentration	(*c*) Not overlapping			(*d*) Overlapping		2	1
Z, color	*A*	*B*	*C*	*D*	*E*	5	4

TABLE 12

Shape	Color "Levels"				
	A	*B*	*C*	*D*	*E*
2×2 square	R	G	B	Y	M
$1\frac{1}{2} \times 1\frac{1}{2}$ square	G	B	Y	M	R
1×1 square	B	Y	M	R	G
2×1 rectangle	Y	M	R	G	B
$1\frac{1}{2} \times 1$ rectangle	M	R	G	B	Y

tions, all different, were made of "cell" *acA*, 6 of *acB*, and so on. We had seen a very large number of these compositions previously, made by some art students at Oxford,[7] and the 120 sample was a set of the most pleasing designs that we had seen, or could make, given the basic principles of Table 11. The general artistic effect of such compositions is quite pleasing, as may be judged by referring to the colored plate in our early mention of the *art-form* test (169).

The operations at issue consist of Q-sortings with the sample. All 120 are shuffled; a subject then looks them through and sets about sorting them on a forced quasi-normal frequency basis, from which

7. The students were in attendance at the Slade and Ruskin art schools, both housed together at Oxford during World War II.

is *most* to which *least* aesthetically pleasing to him. Table 13 shows the forced quasi-normal frequency distribution of scores called for.

THE ARGUMENT

It was argued that effects X and Y would usually be highly significant as judged by F-tests, whereas Z would rarely be so because the compositions are homogeneous with respect to color, each merely involving different areas of the same five colors.[8] Our hypothesis is that persons of high aesthetic sensibility will prefer $b > a$ (Table 11), and $d > c$. The reverse would be the case for unsophisticated persons (aesthetically). It follows logically, therefore, that the order of preference of the compositions for the aesthetically sensitive person is $bd > ad > bc > ac$. By "aesthetic sensibility" we mean the kind of developed taste that fine artists have, as well as persons who have cultivated the finer arts.

TABLE 13

	Most Pleasing									Least Pleasing		
Score	10	9	8	7	6	5	4	3	2	1	0	
Frequency	2	4	8	14	20	24	20	14	8	4	2	(n=120)

PROPOSITIONS FOR THE VARIANCE ANALYSIS

The argument can be written more formally in propositional form as follows:

Proposition 1: Effects X and Y will be significant for almost anyone in the country. (The logic is that of a generalizing inference [90].)

Proposition 2: Effect Z will usually not be significant, that is, more rarely than X and Y.

Proposition 3: Any aesthetically sensible person will provide significance such that, for X and Y, $b > a$ and $d > c$.

Proposition 4: The intrinsic aesthetic possibilities of the compositions will be an order such as $bd > ad > bc > ac$.

(*Definition:* By "aesthetic possibility" we mean the most pleasing pattern that one can *hope* to make. The proposition in no way asserts that "on the average" most Americans will prefer good compositions *bd* above all others of the sample: only the most discerning persons in aesthetic matters will do so.)

8. It would have been quite a simple matter, of course, to compose a sample in which *color* would be significant by F-tests, for most subjects.

We also expect that the interactions will usually not be significant, i.e., for ΣXY, ΣXZ, ΣYZ, or ΣXYZ.

The next step, as far as the variance method is concerned, is to put these propositions into testable form, i.e., to find *operations* by which they may be put to empirical proof. We agree with modern logicians and logical positivists that profound mistakes will be made if propositions of the foregoing kind are examined for their "general implications" (92, 166): it is necessary only to test them under *singular* conditions, that is, to test *singular propositions*.

In Q-methodology the singular propositions are achieved in relation to *variate designs*. Thus we might take *any* person in the country, ask him to make a Q-sort for the $n = 120$ compositions, and so put Propositions 1 and 2 to the test in his case. By the time we have tested 20 persons, any in principle, we shall be confident about conclusions for Propositions 1 and 2. For Proposition 3 it would be sufficient to ask a few artists or persons of cultivated taste to give the Q-sortings, as well as a few persons presumed to be "inartistic" or unsophisticated in matters of art. The variates put together in this way constitute a *variate design*. The one variate design is usually sufficient to put all one's propositions to empirical test. We call Propositions 1–4 "general theoretic" propositions. Corresponding *singular* propositions would be asserted about one's variate design.

Thus, in the present case, we chose 18 persons as subjects of experiment, one *artist*, the *experimenter* (Stephenson), and 16 graduate students. Each gave a Q-sort for the sample, for which he liked *most* to which *least*. This is our variate design. The *singular* propositions are as follows:

Proposition 1.1: Effects X, Y, or both, will be significant for most of the 18 subjects.

Proposition 2.1: Effect Z will be significant, at most, for only one or two of the 18 subjects.

Proposition 3.1: The *artist* will provide significance for X and Y such that $b > a$ and $d > c$. Unsophisticated persons will give the levels in the reverse order.

Proposition 4.1: The order of preference of the artist will be $bd > ad > bc > ac$. (This follows from Proposition 3.1.)

The *singular* propositions are tested by following the usual course of a variance analysis in relation to the structure in the sample. The

variance for any Q-sort is divisible as in Table 14. It is also convenient, however, to regard the four components for *form* (*ac*, *ad*, *bc*, *bd*) as levels of a single effect for form. Table 15 pursues the analysis.

PROPOSITIONS FOR FACTOR ANALYSIS

The Q-sortings for any variate design can be correlated and factored. Under certain conditions main effects, such as X and Y, each for two levels, can be represented by orthogonal factors. Thus

TABLE 14

	D.F.
ΣX	1
ΣY	1
ΣZ	4
ΣXY	1
ΣXZ	4
ΣYZ	4
ΣXYZ	4
Σ Replication	100
Total	119

TABLE 15

	D.F.
ΣX and Y (form)	3
ΣZ (color)	4
Σ Interaction	12
Σ Replication	100
Total	119

X and Y may provide factors α and β, respectively. Positive loadings in α will correspond to $a > b$ of the factorial design, and negative loadings to $b > a$; likewise for β, $c > d$ and d $> c$ will be represented by positive and negative loadings, respectively. When both X and Y are significant for the one variate, it will have both factors α and β appropriately. These correspondences have been proved earlier. But not *all* effects of a design lead to only one corresponding factor, such as $X \supset \alpha$, $Y \supset \beta$. Thus both X and Y were put together to represent an effect for form at four levels, above: we do not in such a case necessarily hope to find a single factor for *form*. Clearly, all depends upon whether bipolar, orthogonal, or other relationships are involved. There is nothing to lead one to suppose, again, that the color effect, if significant, would be for *one* factor only.

The "general theoretic" propositions in the factor case are therefore as follows:

Proposition 1.2: There will be orthogonal factors α and β corresponding to effects X and Y.

Proposition 2.2: A factor or factors corresponding to effect Z will be little in evidence.[9]

Proposition 3.2: Aesthetically sensitive persons will provide factors α and β, with *negative* loadings in each. Unsophisticated persons will give only α, or α and β, with positive loadings.

The 18-variate design gives rise to the corresponding singular testable propositions, namely, that factors α and β will appear for the 18 $\times$ 18 table of correlations; that the *artist* will have loadings $-\alpha$, $-\beta$, and most students $+\alpha$, $+\beta$, or both; that Stephenson

TABLE 16

Source of Variance	Sums of Squares	D.F.	Mean Squares	Expectancy
Σ (form) X and Y..	105	3	35.00	9.6
Σ (color) Z........	19	4	4.75	1.3
Σ (interaction).....	34	12	2.83	....
Σ (replication).....	366	100	3.66	....
Total.........	524	119	....	....

might have $-\alpha$, $-\beta$ (a matter of his information about the issues), and that, if *color* is significant, it may be with respect to more than one principle, in which case it would be represented by more than one factor, which would appear only infrequently with any significance.

RESULTS (VARIANCE ANALYSIS)

Each Q-array was first analyzed by variance method. The data for person No. 1 will serve to illustrate the kind of results: his 120 scores, sorted into their respective 20 "cells" of the design, are given in Appendix II. It is first shown that the replication variances are homogeneous (33): all 18 Q-sorts were satisfactory in this respect.

The analysis for No. 1 is shown in Table 16, using the second form of the factorial design (p. 134), i.e., for *form* (X *and* Y, for levels ac, ad, bc, bd) and color. The F-test gives 3.98 at the 1 per cent level for 3 and 100 D.F., so that only Σ(X and Y) is significant.

9. Effect Z has five levels, which could divide up into one or more orthogonal factors.

The four *means* concerned for $\Sigma(X \text{ and } Y)$ are for 30 compositions in each case (for *ac*, *ad*, *bc*, *bd*). Color is balanced in all alike. No. 1's *means* were as follows:

a c: 4.6 (regular—contiguous)
a d: 6.5 (regular—overlapping)
b c: 3.9 (irregular—contiguous)
b d: 5.0 (irregular—overlapping)

The "population" variance is best estimated from the 100 D.F. for the replications, i.e., 3.66. The standard error of the difference between any two *means* is therefore

$$\sqrt{2}\ \sqrt{\frac{3.66}{30}} = 0.49$$

Setting the limits at $2\frac{1}{2}$ $\text{S.E.}_{\text{diff}}$ gives 1.2, so that differences greater than 1.2 will be regarded as significant. Clearly, component *ad* in the above case is significantly larger than any of the others, which are not significantly different from one another.

His data can now be examined with respect to effects X and Y:

$$\begin{array}{ll} & \text{Means} \\ \text{Effect } X \begin{cases} a \ldots\ldots\ldots 5.55 \\ b \ldots\ldots\ldots 4.50 \end{cases} & \text{difference, } 1.05 \\ \text{Effect } Y \begin{cases} c \ldots\ldots\ldots 4.25 \\ d \ldots\ldots\ldots 5.75 \end{cases} & \text{difference, } 1.50 \end{array}$$

Each *mean* now involves 60 compositions, so that $\text{S.E.}_{\text{diff}}$ is correspondingly smaller, namely, 0.35. The $2\frac{1}{2}$ $\text{S.E.}_{\text{diff}}$ limits give 0.88. Thus both X and Y prove to be significant at greater than this level.

The analysis is complete, and we conclude that person No. 1 prefers *ad* (regular—overlapping) to all other compositions, also *regular* more than irregular and *overlapping* more than contiguous designs.

This analysis is made for each of the 18 persons in turn. *Form* was invariably significant, whereas *color* was rarely so: it occurred significantly for No. 12 and approximated to significance for Nos. 13 and 14.

Table 17 gives the *means* for all 18 persons, for the four components of *form*, i.e., *ac*, *ad*, *bc*, *bd* (color being balanced in each of the 30 compositions of each component). The $\text{S.E.}_{\text{diff}}$ is in the region

of 0.46 in each case: differences of amount 1.20 therefore are regarded as significant.

The analysis for effects X and Y is in Table 18. The $S.E._{diff}$ are now of the order of 0.33, since each component is now for 60 compositions, so that differences of the order of 0.85 are significant.

Customarily, the variates are next classified with respect to their significant results (Table 19). The *artist* was No. 18, Stephenson

TABLE 17

MEANS FOR FORM ($\Sigma[X$ AND $Y]$) FOR 18 SUBJECTS
(Means Are Based on Data for 30 Art-Form Compositions Each)

Persons	Components				$S.E._{diff}$
	a c	*b c*	*a d*	*b d*	
1	4.6	3.9	6.5	5.0	0.49
2	7.2	4.3	6.1	2.5	0.48
3	5.6	5.2	5.8	3.3	0.46
4	3.8	3.8	5.7	6.7	0.42
5	5.7	2.8	6.9	4.6	0.49
6	5.7	3.1	7.0	4.2	0.46
7	2.9	5.4	4.1	7.6	0.44
8	3.1	5.3	5.6	6.0	0.48
9	5.4	4.0	6.4	4.2	0.45
10	3.4	5.6	5.5	5.5	0.42
11	3.3	4.4	6.0	6.3	0.48
12	3.6	6.6	5.2	4.6	0.49
13	5.7	4.6	5.6	4.1	0.46
14	5.2	3.3	6.6	4.9	0.42
15	5.9	5.5	5.3	3.2	0.46
16	4.7	5.1	3.9	6.3	0.48
17	6.7	2.8	6.1	4.4	0.46
18	3.0	4.5	5.8	6.7	0.44

No. 4. It is clear that the various singular propositions are supported. Everyone has either X or Y significant, most both in this case. The *artist* has the expected significances, such that $b > a$, $d > c$. Stephenson, with knowledge of what to expect, reaches the same result. Only four students—Nos. 7, 8, 10, and 11—reach the same result as the artist. The order of preference of the criterion artist is $bd > ad > bc > ac$, as predicted. As far as this experiment goes, then, the initial principles find support. Moreover, *form* is not represented by a single effect, but by two.

RESULTS (FACTOR ANALYSIS)

Table 20 gives the 18 × 18 table of correlations: these are *low*, on the average; but this is as expected, because all the 120 designs are supposedly good examples of their kind, the sample being highly homogeneous with respect to aesthetic quality. The centroid analysis gives factors I–V of Table 21.

A *dependency* rotational analysis is then undertaken. By this we mean that any "cues" are used to reach the required solution. If it

TABLE 18

DATA FOR EFFECTS X AND Y FOR TABLE 6, FOR COLOR EFFECTS (Z) BALANCED IN EACH OF THEIR LEVELS

Persons	Effect X		Differences	Effect Y		Differences	S.E.$_{diff}$
	a	b		c	d		
1*....	11.1	8.9	2.2	8.5	11.5	−3.0	0.35
2.....	13.3	6.8	6.5	10.5	8.6	1.9	0.34
3....	11.4	8.5	2.9	10.8	9.1	1.7	0.32
4.....	9.5	10.5	−1.0	7.6	12.4	−4.8	0.31
5.....	12.6	7.4	5.2	8.5	11.5	−3.0	0.35
6.....	12.7	7.3	5.4	8.8	11.2	−2.4	0.32
7.....	7.0	13.0	−6.0	8.3	11.7	−3.4	0.31
8.....	8.7	11.3	−2.6	8.4	11.6	−3.2	0.34
9.....	11.8	8.2	3.6	9.4	10.6	−1.2	0.32
10.....	8.9	11.1	−2.2	9.0	11.0	−2.0	0.31
11.....	9.3	10.7	−1.4	7.7	12.3	−4.6	0.34
12.....	8.6	11.2	−2.6	10.2	9.8	0.4	0.35
13.....	11.3	8.7	2.6	10.3	9.7	0.6	0.32
14....	11.8	9.2	2.6	8.5	11.5	−3.0	0.30
15.....	11.2	8.7	2.5	11.4	8.5	2.9	0.34
16.....	8.6	11.4	−2.8	9.8	10.2	−0.4	0.35
17.....	12.8	7.2	5.6	9.6	10.5	−0.9	0.32
18.....	8.8	11.2	−2.4	7.5	12.5	−5.0	0.31

* Thus, for No. 1, a is derived from components $a\,c$ and $a\,d$ $(4.6 + 6.5) = 11.1$; b from $b\,c$ and $b\,d$ $(3.9 + 5.0) = 8.9$; c from $a\,c$ and $b\,c$ $(4.6 + 3.9) = 8.5$; d from $a\,d$ and $b\,d$ $(6.5 + 5.0) = 11.5$.

TABLE 19

ΣX Alone Significant	ΣY Alone Significant	Both ΣX and ΣY Significant
$a>b$, Nos. 13, 17	$c>d$, None	$a>b$, $c>d$, Nos. 2, (3), 15
$b>a$, Nos. 12, 16	$d>c$, None	$a>b$, $d>c$, Nos. 1, 5, 6, 9, 14, (17)
		$b>a$, $c>d$, None
		$b>a$, $d>c$, Nos. 4, 7, 8, 10, 11, 18

TABLE 20*

CORRELATION COEFFICIENTS FOR 18 PERSONS
(n = 120 Art-Form Compositions)

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18
1	—																	
2	09	—																
3	01	43	—															
4	16	−36	−18	—														
5	27	43	07	21	—													
6	27	45	25	15	56	—												
7	04	−71	−43	40	−25	−32	—											
8	01	−34	03	36	−01	06	32	—										
9	26	35	21	05	44	34	−22	−02	—									
10	15	−22	−03	19	−05	−08	24	31	06	—								
11	23	−35	−33	37	09	11	39	39	01	28	—							
12	−01	−15	15	−01	−14	−15	10	35	06	41	00	—						
13	00	43	00	−09	34	28	−24	01	15	00	−14	00	—					
14	31	19	08	31	52	43	−13	17	21	−07	27	−15	16	—				
15	00	41	25	−11	03	09	−38	04	06	−09	−17	10	22	22	—			
16	−03	−36	−08	36	−22	−11	28	23	−05	18	13	11	−20	09	05	—		
17	15	50	11	−02	50	50	−38	−25	36	−17	−07	−30	28	38	11	−20	—	
18	22	−51	−27	45	07	04	47	16	−02	26	48	05	−29	20	−30	12	−07	—

* Decimal points are omitted.

can be found, then we can rotate until we reach it. Thus in the present case the results for the variance analysis are available, and it is known that Nos. 13, 17, 12, and 16 are "pure" for effect X, 13 and 17 positively, 12 and 16 negatively; also that everyone has a significant result for effect X. With such matters in mind, it is a simple matter to rotate so as to reach complete agreement, if it exists, between the prior variance results and the factor analysis. We require, first, the two factors to correspond to effects X and Y.

TABLE 21

FACTORS FOR TABLE 20

	CENTROIDS					ROTATED FACTORS				
	I	II	III	IV	V	α	β*	γ	δ	ϵ
1	46	08	−02	−25	13	22	−18	−02	−25	40
2	48	−63	06	31	51	79	38	06	31	33
3	20	−30	32	−21	35	35	28	32	−21	20
4	28	65	−15	−02	−18	−33	−55	−15	−02	34
5	79	−12	−02	10	−13	58	−46	−02	10	32
6	70	−06	−01	16	−12	48	−44	−01	16	30
7	−28	70	−14	04	−32	−72	−37	−14	04	−06
8	07	60	25	01	00	−43	−28	25	01	31
9	53	−10	20	−01	00	41	−24	20	−01	27
10	00	42	49	00	−12	−33	−25	*49*	00	11
11	23	56	−01	−15	−18	−30	−49	−01	−15	26
12	−12	21	69	−12	16	−24	10	*69*	−12	13
13	30	−19	22	59	06	34	−03	22	*59*	12
14	65	20	−16	00	12	25	−32	−16	00	*54*
15	13	−11	14	06	50	17	35	14	06	36
16	−10	46	−02	−19	15	−42	−02	−02	−19	24
17	60	−36	−15	11	−03	65	−18	−15	11	17
18	16	46	−07	−32	−34	−26	−52	−07	−32	07
Σ	5.08	2.47	1.62	.11	.56					

* All signs have been reversed, in order to maintain the convention for $d > c$ relative to $c > d$.

Our solution for the five centroids, maintaining orthogonality, gives factors α, β, γ, δ, and ϵ of Table 21. The rotations are carried out, two factors at a time, as follows: I–II, rotated (anticlockwise) in relation to Nos. 13, 12, and 45° to I′–II′; II′–V, rotated in relation to Nos. 13, 16, 12, and 50° to II″–V′; solution for factors α and β corresponding to X and Y is the rotated factors I′ and II″. We next look at the pairs for III, IV, and V′, and find that three further factors may be supposed, γ, δ, ϵ, with only *one* significant loading on each (for γ it is No. 12; for δ, No. 13; and for ϵ, No. 14). These, it will be recalled, were the variates for which *color* provided

significant results—No. 12 clearly so and the others at the 5 per cent level for the F-test. The agreement between α and β and X and Y is complete (see Table 22).

With respect to effect Z, γ is significant for No. 12, and we are prepared to believe that γ is for *color*. It is possible that different principles of color are involved, about which we have no hypothesis, and that the other factors δ and ϵ are attributable to them. But *interactions* were significant at the 5 per cent level for Nos. 14 and

TABLE 22

ΣX Alone Significant			ΣY Alone Significant	ΣX and ΣY Significant		
	α	β			α	β
$a>b$, Nos. 13, 17	34 65	−03 −18	$c>d$, None	$a>b$, $c>d$, Nos. 2, (3), 15	79 35 17	38 28 35
$b>a$, Nos. 12, 16	−24 −42	10 −02	$d>c$, None	$a>b$, $d>c$, Nos. 1, 5, 6, 9, 14, (17)	22 58 48 41 25 65	−18 −46 −44 −24 −32 −18
				$b>a$, $c>d$	None	
				$b>a$, $d>c$, Nos. 4, 7, 8, 10, 11, 18	−33 −72 −43 −33 −30 −26	−55 −37 −28 −25 −49 −52

10, and these, of course, could lead to factors. Moreover, we did not anticipate any effect of a *general* nature, i.e., common to all 18 variates: factor ϵ could be such an effect. It would represent the *general* aesthetic quality of the compositions, commonly regarded: the loadings are all very small, but all positive.

We conclude that all the singular propositions are supported, and to that extent the "theoretic" ones as well. It is particularly interesting that form requires two *factors* and not *one* and that our principles appear to "work." The agreement between the variance and factor analysis should, in principle, be precise in correctly classifying the variates, and this is true of our example.

DISCUSSION

What, then, has been proved in this example? It is, in the first place, a demonstration that the same results can be reached both ways, by variance or by factor analysis, a matter that Burt (41) has attended to for the covariance situation and not, as here, for the dependency analysis *within* arrays in balanced design. The same results are reached by the two methods in the sense that the variates are similarly classified into groups. But the variance analysis provides facts about the theory at issue, namely, by way of *means* and their differences for specified components of effects and their levels. Any explanation of a factor, in the last analysis, reduces to such matters of differences between means. The factor analysis, on the other hand, deals with the rather more superficial propositions, like Proposition 3.2, if this is put in the form: "Aesthetically sensitive persons will be distinguishable from unsophisticated ones, such as students."

The example, however, will serve best to illustrate Q if it is remembered that we lay our cards on the table, in the form of the structured sample, in relation to which every factor will be explained. This clearly has a *postulatory* feature in attendance upon it, namely, that these explanations constitute our *theory*. Interest is then switched to the more superficial propositions and the complications of the variate designs which represent them. The art of experimenting lies in these variate designs rather than in the designing of samples, although, as we see in the above example, some skill can enter into their construction. The theory, represented by the structure of the sample, is never tested for any general implications—as in R-methodology studies on artists. Work upon a single artist under different experimental conditions is the essential *modus operandi*. One should judge a theory by the number or interest of the singular propositions to which it leads, i.e., the variety of the variate designs which follow deductively from it. In the present example we might argue that young children, because they are not subject to the militating effects of experience and learning, might prefer the more lively designs, for the irregular positions of the colored shapes. This can be tested. Or the underlying theory concerning *form* could be further explored, with different samples. There is never any question of testing large numbers of "cases" for statistical inferences.

But we do not always have structured samples to begin with, and

such as we have may be inadequate on occasion. Many of the most interesting Q-technique studies are undertaken without structured samples. In all such cases, however, we believe that *dependent* forms of factor analysis are essential and can be achieved. We have illustrated above a dependent form of factor analysis in which prior variance analysis is used to govern what the solution will be to the rotational problem; but there are many other ways of doing this, without such prior findings, as we shall see later. In general, if one begins with an unstructured sample, it should be one's object to compose the necessary structured samples as soon as factors have been elaborated and theory is sufficiently formed.

A wrong impression will be given by our example, however, if it is thought that factor and variance methods are merely alternative ways of analyzing data to reach the same results. As we shall see, factor analysis has some genuine *inductive* possibilities open to it in relation to a theory. The classification provided by variance analysis can be reached by an infinite number of different arrangements of factors; and choice of the factors may be all-important. We would also draw attention to the principle to which reference was made in chapter v, namely, that the possibilities or not of dividing an effect into two or more orthogonal components becomes a test of a "primary" factor. In the above example the placing of *X and Y* in *one* effect of a design does not lead to *one* factor, but to *two*, for *X* and *Y*, respectively. There is evidence that *form* alone does not lead to a single factor or effect, whereas the principles of *shape dominance* and *shape concentration* do.

There is some danger, of course, that an investigator might find his results first, and then assert propositions with considerable show of wisdom after the event. One must repeat any experiment, as in any scientific work. Our purpose, however, is to stress the development of theoretical issues, along explicit deductive lines, but also with whatever "hunches" or empirical "know-how" that we may wish to employ.

With respect to our little problem in *aesthetics:* it is true, we believe, that principles of the kind with which we began have never before been subjected to operational tests such as Q-methodology makes possible and that these procedures open up new experimental possibilities for aesthetics. There is not a theory in aesthetics, we believe, that cannot be reduced, as to its main principles, to a

Fisherian design or unstructured sample of some kind. The same applies, however, to psychology very widely. In all cases the methodological principles are the same and are now becoming clear: they are those of postulating explanations or theories as independencies and testing them along dependent lines.

APPENDIX I

Structured Sample for the "Selective Service Dilemma" Study

1. The *factorial design* in question is as follows:

				No.	D.F.
A	(*a*) Reasonable	(*b*) Emotional	(*c*) "Doubtful"	3	2
B	(*d*) Personal	(*e*) Impersonal	(*f*) "Doubtful"	3	2

2. *Combinations:*

	1	2	3	4	5	6	7	8	9
	a d	*a e*	*a f*	*b d*	*b e*	*b f*	*c d*	*c e*	*c f*
Corresponding statements of the "sample"	1–8	9–16	17–24	25–32	33–40	41–48	49–56	57–64	65–72

3. The statements are listed below. It is important to remember that no assertion is anywhere made or implied that any *general* validity applies for these statements so categorized: the sample is merely a sort of best guess, a hypothetical matter, and we fully expect (and indeed hope for it) that the statements will "mean" very different things for different persons in different interactional settings, or for the same person in different settings. The statements are usually typed on small cards, one statement to a card, and numbered in random order. It is important to number the cards *randomly*, so that when they are operated upon by a person, there will be no clues to the structure imposed upon the sample. Thus, the numbering in the following list would *not* be suitable for use in operations with the sample.

4. *Frequency distribution:*

	Least Significant									Most Significant		
X	0	1	2	3	4	5	6	7	8	9	10	
f	2	3	5	8	12	12	12	8	5	3	2	($n=72$)

(*a d*) 1
I should go on with my school-work as long as I can. My schooling can advance me in the services, and I will be that much ahead when I get out.

(*a d*) 2
The best thing for me to do is to obtain as much information as is possible about what I must expect.

(*a d*) 3
I will have to plan carefully. It is easy to make snap judgments with the pressure that is on me now.

(*a d*) 4
Although it is not a pleasant outlook, I am young enough to continue later if I am called.

(*a d*) 5
People are frightened; they try to make decisions too rapidly. The best thing that I can do is to be as patient as possible and see how things really develop.

(*a d*) 6
We who have to face the dilemma are the ones who realize the gravity of the situation.

(*a d*) 7
As a draftee, I must determine whether I am to enlist now of my own free will, or wait indeterminately, for the draft and all its restrictions.

(*a d*) 8
I won't have it easy, regardless of whether I am in or out of the service. Wartime demands will cause many restrictions in civilian living.

(*a e*) 9
The channels of public communication—radio, cinema, newspapers, and television—should be used to disseminate information concerning selective service issues.

(*a e*) 10
By making their general plans as far in advance as possible, and by informing the public as soon as possible, the selective service board can clarify the lives of both those who are not destined for induction, and those who are.

(*a e*) 11
It is difficult to conceive of any equitable policy other than that of drafting all able-bodied men of 18 and 19 years.

(*a e*) 12
The primary question is the relationship between the schooling of the young and their eligibility for service.

(*a e*) 13
What information there is must be made freely available, in a form that everyone can understand.

(*a e*) 14
It is the task of the Defense Department to know how many men are required.

(*a e*) 15
Exempting the scholastically superior student is ill advised and undemocratic, since superior people are needed in the service as well.

(*a e*) 16
Civilian and military controls are essential to a war economy.

(*a f*) 17
A boy in high school has to decide whether to continue his education; and this choice must be made before he even knows whether he will be permitted to complete his education.

(*a f*) 18
What exactly are the issues? Is it not true that we are waging a strategic ideological war?

(*a f*) 19
The young are in a dilemma because they confuse patriotic duty with a realistic facing up to facts.

(*a f*) 20
The situation will ease in the coming months, when selective service may even have become unnecessary.

(*a f*) 21
It is difficult to understand confusion; and if confusion and indecision are the materials, one cannot expect precision and clarity in their presentation.

(*a f*) 22
There is a chasm between the government and the people and within the people itself; this must be bridged, but how?

(*a f*) 23
The confusion covers all phases of current mobilization but is especially pronounced as regards college students' status.

(*a f*) 24
One horn of the "selective service dilemma" arises out of the fact that ours is a democratic society.

(*b d*) 25
You can't blame me for wanting to know if the cause is worthy of laying down my life!

(*b d*) 26
If I only had more knowledge of the government's draft plans, it would relieve much of my tension.

(*b d*) 27
My future has been placed in the hands of federal authority. I can only hope that things turn out for the best for me.

(*b d*) 28
It is difficult for me to remain contentedly at school in all this confusion. The whole mess is leaving me apathetic.

(*b d*) 29
I can't decide whether I should sign up for the next quarter at school or first enjoy myself while there is still some time.

(*b d*) 30
I am wondering about my future, both immediate and distant. It all looks pretty gloomy.

(*b d*) 31
I find myself in a world in which crisis follows crisis, rumor follows rumor. It is awfully discouraging.

(*b d*) 32
How can they blame me for panicking about planning my future, since they give me no basis upon which I can plan?

(*b e*) 33
This is the flower of our country, but no one in Washington makes a decision about us.

(*b e*) 34
Why don't the "BIG WIGS" get together to avoid such a thing as selective service? Does a wife bear children to bring them up fatherless?

(*b e*) 35
The very least the government can do is to tell these young people where they stand, what plans they can make, and how much time they have to make them.

(*b e*) 36
To hear about some of the deliberate subterfuge in the War Department makes me resistant and angry.

(*b e*) 37
I am shocked at the fact that definite, official statements from one quarter are almost always followed, or even accompanied by, a contradictory statement, just as definite and just as efficient, from another source.

(*b e*) 38
Why, in the name of heaven, cannot the ignorami in Washington make lucid and precise decisions with regard to the selective service problem, without changing their minds every month?

(*b e*) 39
Be damned to the bulletins and policy-letters! Write to your congressman and demand it for the sake not only of our youth, but of an effective defense-democracy; demand that a forthright announcement be made of just what our selective service policy really is.

(*b e*) 40
It scares me to think that we slough off the basic questions asked by our youth with vague generalizations or propaganda slogans. We must face the issues squarely, even if our answer is as yet unclear.

(*b f*) 41
Why must America's youth be set in turmoil because of selfish interests?

(*b f*) 42
It is bewildering to probe into the controversy in hand.

(*b f*) 43
It is enough to break anyone's nerve to live in suspense and confusion.

(*b f*) 44
It is toward death that many of our young are heading; why, then, need they worry?

(*b f*) 45
By following the way of hate, man has come to this mess.

(*b f*) 46
One reason why youth is restless is because they are unable to look ahead into the future; it is all a blank!

(*b f*) 47
Why does such a state of affairs have to exist; it makes life seem futile.

(*b f*) 48
It is all one big mess; no one knows what is happening.

(*c d*) 49
The masses are hysterically waving their flags; as a young man I must wave mine, too, but which way?

(*c d*) 50
I need more personal contact with draft officials; they might clear up some of my misconceptions.

(*c d*) 51
Although my whole future is tied up in the present emergency, no one seems to be giving me the help and guidance I need.

(*c d*) 52
Eventually I become adjusted to things as they are; but it is hard to see how a world crisis can be avoided.

(*c d*) 53
How can I continue in my school work?—when I don't know what comes next!

(*c d*) 54
My lot is hanging in the balance; it will be some time before I know where I am going.

(*c d*) 55
I am ready to defend my country and have a right to know just what will be asked of me and when.

(*c d*) 56
I am sick of the rumors and half-truths that reach me from every source.

(*c e*) 57
Assuming the governmental policies are clear and precise, as, for instance, in the case of draft age, how should the decisions be disseminated and made palatable to the common mind?

(*c e*) 58
To maintain a regular flow of college personnel, by means of selection policy, or rather to vote for equality of service, that is the question!

(*c e*) 59
We frankly doubt whether America should raise a large army at this time.

(*c e*) 60
There should be some plan to maintain the flow of persons with higher education; e.g., selection or rotation of office.

(*c e*) 61
The status of married men and National Guards men has not been explicitly stated. The government should take immediate steps to clarify these issues.

(*c e*) 62
Is it U.S. policy to act as part of the UN police force?

(*c e*) 63
There are questions related to the status of college students; these need to be settled, too!

(*c e*) 64

Does this country want peace so that it won't be bothered in its policy of business and exploitation of underprivileged countries; or will it help these nations?

(*c f*) 65

Because our youths' questions are basic, they must be faced squarely. How can we act as ostriches before questions like these?

(*c f*) 66

The situation may be explained in a parable: A young man and an old one were talking when a khaki dragon came along and ate up the young one, leaving the older one alone. Such is selective service.

(*c f*) 67

What exactly are the issues that must be fought for?

(*c f*) 68

The world-situation is grave for no fault of ours!

(*c f*) 69

Patriotism is not an excuse for war and excess; it means high loyalty to the common good.

(*c f*) 70

The confusion is unparalleled.

(*c f*) 71

The young people of our country are in this dilemma not because they want it but because it is considered a patriotic duty.

(*c f*) 72

More than ever before, knowledge is needed; more than ever before, there is only ignorance.

APPENDIX II

Scores Given by Subject No. 1 (Table 19) to the 120 Compositions of the Structured Sample

Forms	Color Combinations: A	B	C	D	E
	Regular Contiguous $(a+c)$				
X	6 6 6 3 5 4 (T_1)	6 5 1 6 6 4 (T_2)	5 8 5 8 1 3 (T_3)	6 6 1 3 6 2 (T_4)	8 5 2 5 3 4 (T_5)
	Irregular Contiguous $(b+c)$				
X	3 2 3 4 0 4 (T_6)	5 2 7 4 6 5 (T_7)	5 5 5 5 3 5 (T_8)	3 4 5 4 5 3 (T_9)	5 3 3 4 3 2 (T_{10})
	Regular Overlapping $(a+d)$				
Y	0 3 9 7 8 6 (T_{11})	7 7 5 6 6 4 (T_{12})	7 9 7 8 6 7 (T_{13})	7 8 7 4 7 7 (T_{14})	10 2 8 6 9 7 (T_{15})
	Irregular Overlapping$(b+d)$				
Y	4 4 7 5 5 6 (T_{16})	3 4 4 6 1 5 (T_{17})	4 4 6 8 4 10 (T_{18})	4 4 5 5 6 7 (T_{19})	5 6 5 2 2 9 (T_{20})

PART II

PRACTICAL APPLICATIONS

THE concern, so far, has been with general principles upon which Q-methodology is based. No summary of these is given at this point: if the reader wishes to pause awhile and look at Q in brief perspective, he will find the chapter at the end of this book suited, perhaps, to his needs. Meanwhile, it is sufficient to observe that, with the various principles in mind, we propose to venture into many regions of psychology with an inquiring, methodological purpose. The kind of analysis we made into the more abstract principles upon which the psychological and social sciences have been based is now to be repeated in the concrete settings of self, social, clinical, and other branches of these sciences. We shall find that our conclusions are just as surprising as they were for the general principles and that, as we might expect, the procedure of Q-technique itself serves to illuminate these conclusions with considerable pertinency.

In the early account of Q-technique given in Sir Godfrey Thomson's *The Factorial Analysis of Human Ability*, in which he relates how he and the present author drew attention independently and almost simultaneously to it, Sir Godfrey was pessimistic, whereas we were highly optimistic. The reasons for our own enthusiasm are now to appear, if they are not already apparent. With James Ward, we believed that "all experience is experiment"—but one would say nowadays that all *behavior* is. We could see the possibilities of applying Q-technique to problems, almost everywhere in psychology, which had hitherto been untouched by operations of any kind. "The whole domain of psychology," we wrote in 1935, "is open for the technique to explore." But the reasons for our concern with so simple a matter as Q-sorting (a procedure which, when all is said and done, is intrinsically almost as simple as licking a stamp or reciting a nursery rhyme) lay, if we may say so, in a certain broad grasp of psychology, in its systematic and historical aspects, that Spearman gave to his students when the present writer was one of

them. Our interests, therefore, have been in psychology in its widest aspects, in all its branches. The grasp we have may seem too sweeping, perhaps, and too historically oriented at times. That a great deal of patient empirical work has to be done, and can be done if our thesis is correct, is quite certain. We hope, however, that the many experts in the various fields of psychology, into whose difficult subjects we propose to thrust our way, will forgive us for many cavalier incisions and peremptory intrusions: our rapier is sharp at times, but not too long. Moreover, we expect many a riposte.

Part II, however, should be prefaced with a word or two of caution about the hypothetico-deductive method. The hallmark of sound scientific procedure nowadays, it seems, is to assert hypotheses and to confirm predictions. Everything in Part II in the chapters to follow is directed toward such a scientific attitude and, certainly, toward the frank acceptance of theoretical standpoints and to experimenting with their consequences. There is need, however, for care and discernment in these matters. What one person will regard as a theory may be little other than a system of logical possibilities, arbitrarily analytical in relation to more essential problems. A theory which is intrinsic to the demands of a situation is one thing, and one imposed upon behavior is another. There is a tendency nowadays to regard experimentation in psychology as a sort of chess game, in which rules are postulated, deductions drawn, and the logic put to empirical test. Our instincts are against such an attitude. Psychology, it seems to us, has by no means achieved a sophisticated theoretical status, with ideal constructs such as physics has fashioned for itself. The situations in psychology, therefore, call for an attitude of *curiosity*, as well as one of hypothetico-deductive logic. A somewhat detached, but inquiring, attitude is called for, in which one seeks to learn more about the intrinsic empirical possibilities rather than the purely logical, deductive, or carefully reasoned ones. We should be making discoveries rather than testing our reasoning. It is indeed remarkable, as Kohler (98) has noted, that psychology, teeming as it is with investigators, is not altogether overburdened with important scientific discoveries. We suspect, and have much evidence of it, that logical possibilities cannot replace the patient muddling of the empirical investigator.

That is, one's scientific judgment is at issue. These chapters offer the possibility of representing theories and hypotheses almost *ad lib*

in relation to structured samples. We like theory, and no one could suppose for a moment that ours is merely a fact-finding device or that our scientific philosophy is one that calls for more and more facts about everything under the sun. Indeed, as much as anyone, we believe that one must know *what* to look for and that therefore one must have some kind of theoretical expectancy. But these are early days in the study of behavior, and, although many gains have been made, notably in the realization that the old psychological rubrics of intelligence, problem-solving, memory, social interaction, perception, and all else merely subsume behavior in the raw, it is of the utmost importance to look at the teeming behaviors confronting us with a fresh and puzzled attitude, willing to take what comes along in terms of very general considerations to start with, believing nothing, and expecting little. Indeed, just because these new methods are placed at our disposal, the need is all the greater for sound judgment in these directions. It is still true that discoveries do not come from formal deductive arguments except in a highly sophisticated science, and even there nature and reality are likely to confound the theoretical scientist. We believe that an attitude of mind in which empirical curiosity is supreme, rather than hypothetico-deductive logic, must be the mainspring of behavioral science, as it is of Q-methodology.

This usually means that, instead of asserting postulates X, Y, Z, . . . , and deriving from these some testable propositions a, b, c, . . . , the situations in psychology call for the study of X, Y, and Z more directly, as general theoretic propositions, each giving rise to many singular propositions in its own right. The conviction must come that something interesting and empirically important has been found. Curiosity should govern all else, the hypothetico-deductive methodology being a servant and never the master of science.

We now proceed, then, to consider applications of Q-technique in many different problem areas in psychology. Much will have to be omitted for lack of space. Most attention will be given to applications within the rubric of subjective psychology, although it would have been as easy to elaborate upon the applications of Q to the study of behavior as outwardly regarded: examples of such studies, for example, of *performance* as such (Stephenson, 161), are already in the literature, and we therefore make no further reference to them here.

CHAPTER VIII

APPLICATION TO TYPE PSYCHOLOGY

OBJECTIVES

WE MAKE a beginning with a venture into *type* psychology, largely because some of the early applications of Q-technique were for Jung's famous theory of *personality* types (87). Our purpose, however, is not to provide a rationale or defense of any particular type psychology, such as Jung's, Spranger's (152), or Kretschmer's (101), but to use one of these as an excuse for some consideration of methodological issues and to afford opportunities for presenting some of the statistical procedures used in Q in a concrete setting. We propose to serve these two ends and, at the same time, to continue our exposition of the thesis that theories are testable for their singular propositions. The position with regard to *type* psychology is particularly interesting, methodologically, for R supports one standpoint about types, and Q a wholly different one. The psychological textbooks are likely to say that types do not exist, whereas they are an everyday occurrence in Q. Only Stagner (153) has drawn attention to our early standpoint about these matters. We begin, therefore, with some consideration of Jung's theory.

JUNG'S THEORY OF PERSONALITY TYPES

Jung's central concept is one of *individuation*, that is, the impulse of a person to distinguish himself as a "single, separate, person" from the collective mass of others around him. The introvert and extrovert attitudes are the most general forms of this process; these were conceived as outcrops or resultants from the cultural and social-psychological interactional settings in which the person is reacting. It follows, therefore, that in one setting a person may be introverted and in another extroverted, although most habitually he may tend to cling to one type in almost all fields, as a "style" of behavior. The individual should be of neither type in any habitual way, however, if he is truly and adequately "individuated" (to coin a word); only in this way can we understand Jung's references to

the bifurcation into introvert and extrovert types as in some sense a failure of man to adjust himself properly. Schiller is quoted as having said that "it was culture itself that dealt this wound to modern man," i.e., making the split into the two habitual types. But man's abilities or psychological functions also mediate in the individuation, and Jung adds, on this account, his well-known quaternary, the Thinking, Feeling, Intuition, and Sensation functions. Similarly, he has to find a place for conscious and unconscious reactions, and he notes, in this connection, the tendency for a conscious function to be attended by a contrasting "inferior" function in unconscious reactions: the dominantly thinking extrovert, for example, gives himself away unconsciously by many feeling-type responses.

Broadly, then, two main types are distinguished, but no one is possessed of the one attitude with complete atrophy of the other. The two types are "of such a superficial and inclusive nature that it permits of no more than a rather general discrimination. A more exact investigation . . . yields great differences between individuals who nonetheless belong to the same group" (Jung, 87). Difficulties in placing a person in his proper type are referred to by Jung, such as that due to the process of compensation, whereby an Origen (basically a feeling extrovert) castrates himself and assumes the thinking type instead. Corollaries of some interest are also given: "fantasy," for example, is held to bridge the broken gap between the claims of introversion and extroversion. It is said that the extrovert has a certain "repugnance, fear, or silent scorn" for introversion, as the introvert has no less for extroversion; and, although the two main types can be distinguished with ease, according to Jung a sound discrimination of the functional additions requires, instead, a "very wide experience." There are many of these inferences dotted through the pages of Jung's writing.

THE THEORY IN R-METHODOLOGY

The R-factorist's logic consists of replacing the theory by a single general proposition. Thus, with respect to Jung's theory, the assertion was of the following order:

All persons are introverts or extroverts in some degree habitually. . . . (i)

It will be said: "But, of course, the R-factorist was merely hoping to prove or disprove such a proposition." At first sight it seems reasonable to call for a crucial test of Jung's theory about introverts and

extroverts in terms of individual differences; and all the many attempts to measure introversion-extroversion, in such scales as the Bernreuter or the Neymann-Kulstadt, or in the studies of the Guilfords (75), Abernethy (1), and many others, as well as the related works on *perseveration* in the Spearman school (148), all clearly had in mind the notion that, if Jung was correct, it should be possible to isolate and to measure a general introversion-extroversion function in terms of individual differences. The goal was *one* such function if possible, or, if not, then R-technique factor analysis would indicate how many components "it" must be supposed to have, instead of the one. The Guilfords (75), as is well known, found quite a number of *factors* instead of one.

Even if one universal factor is not acceptable and others have to be adumbrated, as in the case of the Guilfords' studies, the implication is still that general propositions are at issue. But formulations about general propositions, *as testable matters*, are now known to be based upon a misconception of the status of *laws* and of a *theory* (92). Obviously, we can never verify general implications of this kind, nor can we find a person who is neither introvertive nor extrovertive in any degree.

The distinction between a rational law and an empirical or scientific one is an old one: it is well described, for example, in Spearman's *Psychology down the Ages* (150), and it is important to distinguish between the logical matters at issue. A theory is often stated in the form of a rational law or laws, with supporting postulates and definitions; the term "scientific law" is perhaps best reserved for quantitative, i.e., operationally defined conditions—a rather different matter. Thus isomorphism is a rational theory, but $S = k \cdot \log R$ is a scientific law. Theories and laws alike, however, are to be regarded as schemata, or models, from which many singular propositions may be constructed, and it is these that are put to empirical test. As Schlick (140) puts it:

> Natural laws do not have the character of propositions which are true or false, but rather set forth instructions for the formulation of such propositions. . . . Natural laws are not "general implications," because they cannot be verified for all cases; they are rather directions, rules of behavior, for the investigator to find his way about in reality, to anticipate certain events.

Theories, for us, have the properties given to natural laws in Schlick's adumbration. We are not ready to assert that our theories

in psychology have the status of natural laws; but they have the same presumptions, point in that direction, and involve precisely the same scientific methodology. For us, along Q-technique lines, theories are explanations, as we saw in Part I, serving as growing points for singular propositions. Theory is represented in Q-samples, ready to offer explanations for any factors we may wish to consider. Problems are tackled by way of *variate designs* (p. 133).

A theory is then accepted or rejected, in due course, in relation to different tested propositions and never in terms of any general proposition. In this connection we follow the usual practices in scientific method (92). There are always the *basic elements* of scientific procedure to consider—whether a proposition is accepted or not depends upon empirical grounds, but a *decision* is involved at this point, which the scientist makes, and none of these decisions is ever irreversible. The scientist can change his mind. The decisions are always in relation to a *total scientific situation*, by which we mean relative to the totality of propositions at the time. These terms, "decision" and "scientific situation," have technical meanings. Thus there is a difference between mathematics and logic, on the one hand, and an empirical result, on the other: decisions in the former are irreversible, those in the latter always reversible; $2 + 2 = 4$ is a proposition impossible to deny with the usual definitions of our numerical system; but the proposition "Joan is neurotic" has nothing so certain about it. We might, on some particular empirical grounds, accept the proposition for the time being about Joan, relative to many other propositions that we have *also* accepted—such as her age, her social status, and the like. But the proposition may be rejected later, on new empirical grounds, and again relative to the totality of all other propositions available about Joan at the time. Similarly, then, with respect to Q: singular propositions are always being empirically tested, and whether they are confirmed, falsified, or tempered, decisions are taken about them relative to the pertinent scientific situation. In R, on the other hand, theories are not dealt with in this way. They are not tested under singular conditions, nor are decisions in relation to a wide scientific situation. Theories in R are tested only for their general implications, as though something of a logical kind, such as $2 + 2 = 4$, is at issue as an empirical matter.

THE THEORY IN Q-METHODOLOGY

We have already seen how Jung's theory can be represented, as to its presumed independencies, in a structured sample (pp. 69 f.). The main effects, for *introversion-extroversion*, for *mechanisms*, and the *functions*, are readily fitted into a balanced Fisherian design. An example of a structured sample for the theory is given in the appendix to chapter iv. With respect to problems, dotted through the pages of Jung's classic (87) there are 80 or more "general theoretic" propositions of the kind we require, from which singular propositions can be asserted. None of them have ever been tested before, in spite of all the efforts of those who have made introversion-extroversion scales. It will be sufficient here to list only a few of them, to give something of their quality.

PROPOSITIONS

1. Given any person X, he or she is introvert or extrovert, or neither habitually, as a "rather general discrimination."

   This would be amplified to mean either (*a*) from X's internal frame of reference, or (*b*) from the external frame, or (*c*) from the historical frame. Each would provide its own singular testable proposition.

2. Given any small number of persons, say 20, introvert and extrovert *person-types* will occur.

3. These will be related, however, to stereotyped or cultural notions of "ideal" introvert and extrovert types.

   It is a simple matter to show that these cultural stereotypes exist: one merely has to invite any subjects, X, Y, Z, . . . , to give descriptions of (i) an "ideal" introvert, and, subsequently, (ii) an "ideal" extrovert. The two are likely to be bipolar for a single Q-factor.

4. More exact investigation will show "great differences between individuals who nonetheless belong to the same type" (Jung, 87).

   The *type* involves a *common* factor or factors only, whereas the individual's full array usually involves *specificity* as well. The latter may constitute 50 per cent of the available variance and requires its own explanation, usually a matter related to *unconscious* mechanisms, as it happens.

5. Children X, Y, and Z of sophisticated parents A and B, will be less "individuated" than the parents.

   The *one* family provides the necessary variates for a factor study which can provide a detailed answer to this proposition. A different answer might occur for every family so tested, and yet in each case the evidence would clearly support the proposition, which in no way asserts that the same kind of "individuation" will occur in all families.

6. Extroverts X, Y, Z, . . . , will have more *insight* into another extrovert W, than a number of introverts, A, B, C, . . . , will have.

   A test of this has been given elsewhere (174).

7. The process of "compensation" (Origen, remember, *assumed* a change of function) can be indicated for any particular person X.

   The proposition can be put to test in terms of several conditions of instruction for X to operate upon, with respect to a "sample" of Jungian statements.

8. Fantasy is the "bridge" between a person X's claims of introversion and and extroversion.

   X can be asked to assess himself under various conditions of instruction, and also to do so in terms of what he would like his "best friend" to be like, or what the character "he admires most in literature" is like, and so on. The art of experimenting consists of designing the variates to answer problems of this kind.

9. There is a relation between *perseveration* and *introversion-extroversion* person-typification.

   A sample of statements (S_1) on perseveration (of ideas) and another (S_2) on Jungian lines will provide the basis of self-descriptions by any experimental persons X, Y, Z, ... Persons of a type with respect to S_1 should be the same as those of a type with respect to S_2.

10. The inference can be drawn that if we were to test proposition 2 for a large number of persons, the shape of the distributions for introversion-extroversion person-types will be unlike anything described hitherto in textbooks.

It was thought by R-factorists that one had first to find an objective method of measuring introversion-extroversion before those propositions could be tested. In Q-terms, however, each can lead to singular propositions, or propositional sets, which are testable in relation to the theory by way of suitable variate designs. How to put proposition No. 1 to empirical test has been referred to already in chapter vii. The experiment on our landlady is the very simplest variate design conceivable in Q. The other propositions, Nos. 2–10, would require, each, its own variate design or designs, and we shall look at some shortly. First, however, a few details of definition and methodology require brief attention.

DEFINITION OF TYPES

It is well known that differential psychology, the science of individual differences or R-methodology, has long denied any general validity to psychological *types*, and type theory is regarded in this country as almost archaic and outdated. Q-technique, on the con-

trary, brings types to light with great ease, and makes one at least wonder about them.

The word "type" has two meanings:

i) It may mean a "person, serving as a characteristic specimen of a class."
ii) Or it might mean "a *class of persons* having a common characteristic or characteristics."

Much confusion attends the use of these two definitions in psychology. R-technique has restricted its operations to meaning ii; most typologists were thinking of i, and Q-technique deals with i also.

The selection of a person to be characteristic of a class, after the class is defined, may be along three lines:

a) Any person might be chosen to represent it, just as any baldheaded man could be chosen as a "characteristic" specimen of baldicoots.
b) Usually some kind of *average* person, or *modal* person, is chosen.
c) Or someone is chosen who is the very *essence* of the class, who is more than the class, yet distinctive of it in some way.

We seriously doubt whether anyone would care to select an "extreme" case—by definition unlike the class rather than like it—to be representative of a class.

It is natural to suppose that a *class* of persons must be discovered or defined before a specimen can be chosen from it to represent the class; and this no doubt is the usual state of affairs. But the opposite may occur. A specimen may be defined first, and thereupon a class may become apparent. Something of the kind is achieved, perhaps, by famous novelists or poets who provide a description of a character and bring to light a class of persons hitherto overlooked. Mr. Pickwick is no doubt a figment of Dickens' imagination: but he happened to be the essence, perhaps, of a trend; he is a caricature, no doubt, but nevertheless distinctive in some way of a class of bewildered and well-meaning Englishmen of the time. The character defined such a class and not vice versa. Some sociologists have discussed possibilities of this kind, e.g., Karl Mannheim (112).

Our language places us in difficulties vis-à-vis such distinctions as *any*, *average*, *modal*, *typical*, and *distinctive*. But if we continue the Dickensian illustration, Pickwick was no *any*, no *average* of all bewildered Englishmen, no *typical* one either: he was an essence, the very choicest specimen imaginable of the kind. Cases *a* and *b* arise when a class is already defined; case *c may* precede the definition of

a class. Two kinds of *distinctive* specimens have to be distinguished, one having reference to an *extreme* case of a predefined class, and the other an essence that precedes the definition of a class. The "objective" psychologist will no doubt find the latter a very difficult conception to swallow. But it is worth a moment's further regard.

Winston Churchill, for example, is widely regarded as typically British, if not, indeed, *the* Britisher par excellence. He is the very essence of things English, of the hard-living, practical-idealistic, richly enthusiastic type. Scotland cannot produce his kind, nor Wales, nor Ireland. Or so it seems. Churchill is *Britannus rubicundus*, very much a person and a specimen of the choicest kind. Another type, *Britannus vacuus* is of a different form: one thinks of the pale, vague, bemonocled, languid Englishman of American caricature, who is rather stupid, you know, but jolly decent. There might be only one choice specimen of *rubicundus*, and a few more *vacui* existing at one time in England; Scotland, however, could never provide an authentic case of either type.

A typology such as Spranger's had to do with just such essences of a culture or a social milieu, and Jung almost grasps something of the same kind. Very complex patterns of behavior are involved, individual and social, and not mere cuts along a unidimensional capacity. The essence of baldheaded men involves far more than the specimen's shining pate: every action of the specimen would be reflected in his transcending dome—his benign fatuity, his prodigious blandness, and his rounded waddle: even his clothes would follow suit.

The widespread belief that types are merely the extremes of a normal distribution, and therefore that no "pure" types exist, is based upon R-technique and nomothetic postulates and proto-postulates. The belief, of course, is mistaken. We should distinguish between the following concepts vis-à-vis types:

In R-technique:
- i) scission types
- ii) supraordinate types
- iii) profile-types

In Q-technique:
- iv) classes of variates
- v) interactional person-types

i) *Scission types.*—These are defined by cuts across normal or other distributions on a single continuum. Thus *idiots*, *morons*,

feeble-minded, *normal*, *superior*, and *talented* can be defined by cuts across a scale of intelligence. *Introversion-extroversion* was widely so regarded. Any *factor* in R-technique can give rise to such types, by definition.

ii) *Supraordinate types.*—If there are m orthogonal factors in R-technique, each divisible into R scissions, there result R^m possible supraordinate types. Thus, if there are 7 primaries for *temperament*, as in Thurstone's work (191), and if each is divisible only into *above* and *below* average ($R = 2$), the number of possible supraordinates is $2^7 = 128$.

Thus, for the 3 primaries, impulsiveness, sociability, and reflectiveness, where $R = 2$, there are *8* supraordinates (Table 1).

TABLE 1

Primaries	Above Average	Below Average
Impulsiveness. .	*a*	*b*
Sociability.	*c*	*d*
Reflectiveness. .	*e*	*f*

Supraordinates							
1	2	3	4	5	6	7	8
a	*a*	*a*	*a*	*b*	*b*	*b*	*b*
c	*c*	*d*	*d*	*c*	*c*	*d*	*d*
e	*f*	*e*	*f*	*e*	*f*	*e*	*f*

iii) *Profile-types.*—These either are unsystematic forms of supraordinates or else issue as "person"-types from our *system 3* forms of analysis (see pp. 51 f.), that is, from transposes of R-technique. In both cases the items at issue are not in interaction but are measured according to the "rule of the single variable." Thus in R-technique there may be M variates (*tests*) and N persons. Each variate is quantified according to the "rule of the single variable"—each test is applied, of course, separately to the N persons. If the M variates are correlated (as in P-technique, or in *system 3* generally), types of persons may be defined in terms of factors for the *tests*. We call these "*profile* person-types." Similarly, any number of variates may be defined, such as for (*a*) intelligence, (*b*) emotional stability, (*c*) socio-economic level, etc., and standard or other scores on these may be ranked, or otherwise grouped, leading to profile-types for persons with similar patterns of scores.

In the cases of types i and ii, it is not the *persons* who should have been typed or classified but the tests or other variates. Thurstone's

seven primary factors for temperament tell us that seven classes of *statements* can be defined, namely, those subsumed by the factors. Similarly, it is sensible enough to talk about a nonverbal type of mental test. In R-technique the factors represent classes of tests or the like variates, and these can be relatively few in number and may be uncorrelated. Specimen tests or statements of any such class may be chosen to represent the classes. But typification of *persons* leads to an overwhelming proliferation of types if several factors are at issue. With respect to type iii, there is a superficial similarity between these and the person-types that issue from Q-technique: the differences between them, however, are obvious—in Q-technique *interactions* are involved and *theory* about personality; in the R-technique case there is no possibility of such interactions, and no theoretical matters are at issue.

iv) *Classes of variates in Q.*—It is clear that each factor in Q represents a class of variates. The concept of *simplest structure* indicates that variates, however, may be "pure" for a factor or main effect or that they may be "mixed" for two or more factors. The word "type" is sometimes used synonymously with "class of variate": a pure type is thus a class of variates involving only one factor, and a mixed type one involving two or more factors significantly. In this sense types abound in psychology, pure or mixed. The proportion of persons of a given population who are of one class or another is a matter for empirical observation—the frequencies have to be counted. No one could say a priori that there will be equal proportions of the population for each class, pure and mixed, or that normal distributions are involved. On the contrary, the "pure" classes are uncorrelated, i.e., orthogonal to one another, and not distributed on a univariate scale of any kind. The classes at issue in this way are for every conceivable region of psychological operations—in every branch of psychology where behavior is under investigation. Thus the experiment on the *art-form* test (chap. vii) pointed to three or four classes of variates, each of which could be indicative of a type of person with respect to aesthetic matters. The study of the *selective service dilemma* suggested three classes or types of men. The word "type" is ubiquitous in this way.

It is important to note that each *factor*, or each *class* of variate (where two or more factors are involved), always entails all N items

of the sample, *in one operational setting for each variate.* Given a number of statements, *a, b, c, d, . . .* , the concern is with the way these are interrelated in given settings or interactional situations. The situation is clear if we think of a Q-study of *M tests* (of the kind used in R-technique): a person X could appraise these for such matters as "Which of the tests would you prefer to do?" or "Which do you think you can do best at?" All *M* items confront X in the one setting, and he appraises them relative to one another. In R-technique each test would be taken, instead, one at a time and worked at according to the postulates and definitions of that technique, everything possible being done to insure that what X does at one test will not affect what he does at another in the one setting. Classes or types in Q, on the contrary, represent complex relationships. Each is often comparable to a "whole" description of a person.

v) *Interactional person-types.*—The *types* discussed by Jung, as we said long ago (164), are of category iv above, and not at all of the *scission* kind. Jung compared his types to the outcome of composite photography. Galton took front-view photographs of women's faces and then superimposed the negatives upon each other. The composite features show through and can be developed as a photograph. Katz (91) has shown that remarkably few such negatives are needed (as few as *ten*) to reach a stable composite under certain conditions. Similarly, then, for types in Q. The statements of a sample are superimposed, in an analogous manner, when we sum their scores to reach the *factor-array* for a factor. The factor consists of the statements, arranged in a certain order that "shows through." *All* the statements are involved in every factor, just as every detail of feature is involved in a composite photograph. When persons have given Q-arrays, their factors are themselves precisely such Q-arrays, which a person *could*, in fact, have given. All these conditions are very different from those dealt with as *scission* classes.

We wish for the moment to reserve a special category for type-psychologists such as Jung or Spranger, however, because the concern has been more particularly with personality theory, and special conditions may be involved in the empirical proving of the theories at issue. Spranger's theory, for example, is *his* conception of behavior rather than what might be supported by operations, in its terms, by the persons he observed. To reach Jung's typology, it may be im-

portant to take account, first, of cultural stereotypes in terms of which individuals operate. Certainly, not all types or classes of variates we deal with in Q-method correspond to Jung's, Spranger's or any other type-psychologist's classes. For the present, therefore, we shall reserve a category of type for the type-psychologists proper, although, methodologically, all are of variety iv.

CONCLUSION ON TYPES

It is as well, therefore, to distinguish the following:

1. Scission types are artificial cuts across a monotonic or other scale, a matter of R-technique.
2. Supraordinate classes are logical combinations of scission types, and also a matter of R-technique.
3. Profile-types are related to the supraordinates and are also a matter of R-technique.
4. In each of the above, the concern is *not* with *interactions.*
5. Interactional person-variate-types, or "person-types" for short, are factor-arrays in Q-technique.
6. These may be "pure" for a factor or "mixed" with respect to two or more factors, as is the case for all factor-arrays in Q (see p. 108).

One of the important consequences of these definitions is that factors issuing from a small number of variates in Q-technique may define person-types as adequately as could be done from very large numbers, just as Katz has found for his composite photographs of faces.

The logic of the R-factorist was at fault, therefore, in several ways when he approached *type*-psychologies such as Jung's or Spranger's. He failed to distinguish between scission, profile, and interaction person-types. He conceived of the theories in terms of general propositions, whereas only rational theories were at issue. He also did not distinguish between specimens as "essences" and "examples of" the types that he had, in fact, adumbrated. Nor was it realized that an experimental methodology was required to determine what the type theories held in them for psychology.

SOME EXPERIMENTAL PROCEDURES

It is now our object to illustrate some of the more detailed statistical and methodological matters involved in Q-technique, and, since

we have a list of propositions already for Jung's theory, it is as well to base our examples upon these. Proposition No. 1 (see p. 157) has already received attention, it will be remembered, for our landlady. It can be tested, of course, for anyone. No individual differences are anywhere postulated. Instead, they are discovered.

Before leaving the landlady, we should interject an additional little experiment performed upon her. Some days after describing herself in the manner outlined in chapter vii, we asked Mrs. X if she had ever heard of an introvert and of an extrovert. Her reply was that she had, and she added: "You are probably an extrovert, I suppose, although you study too much." We next invited her to describe with the two sets of 80 statements, as she had done for her self-appraisals (*S*), the following:

(*E*) What she thought she would be like if she happened to be an ideal extrovert.
(*I*) What she thought she would be like if she happened to be an ideal introvert.

Her three assessments for *S*, *E*, and *I* can be correlated, of course, and her results are given in Table 2. That is, Mrs. X really has a firm

TABLE 2

	S	*I*	*E*
S	....	48	−31
I		....	−86
E			....

(n = 160 statements)

notion of what she thinks an ideal introvert is, and it correlates positively with her own self-assessment made earlier without prior knowledge of what is involved; she correlates negatively with her own idea of what an ideal or "typical" extrovert is; her own two "ideal" representations correlate highly negatively, as we might expect. But each "ideal" can be handled by way of variance analysis, in detail. It is unnecessary to take it further along those lines, however, for sufficient has been given to illustrate the matters at issue.

Clearly, any number of naïve persons, like Mrs. X, can be invited to offer descriptions of themselves in terms of the sample of statements: in each case what is statistically significant can be determined

by variance analysis. Any persons with similar significant effects, in the same direction, can be regarded as persons of a type, i.e., an interactional *person-type*. So far *no* factor analysis would have been involved: yet a beginning would have been made, with the effect of showing how far the subjects (1) were significantly introverted or extroverted according to Jung, and (2) how far they differed with respect to the *functions*, etc., again according to Jung. Up to this point certain facts would have been demonstrated, quite securely, which Jung would apparently explain along the lines of his theory. But, of course, *other* explanations may be forthcoming.

We arranged for fifteen naïve persons to repeat the experiment already noted for Mrs. X, using the same 160 statements. All were women. In thirteen cases, effect X (see p. 69) was significant, and, of these, eleven gave high scores to *introvert* statements and two to *extrovert*. The effect Y, for conscious or unconscious mechanisms, was in no case significant; but effect Z (the Jungian functions) were variously significant for the different women, centering, however, mainly on the *feeling* function. Without any correlational factorial analysis at all, some of Jung's propositions have already been put to the test, at least up to a point of fact, namely:

i) These women fall into two main classes, *in terms of Jung's statements*, that is, into introvertive and extrovertive person-types.
ii) Women are more likely to be introvertive than extrovertive (this, of course, would require *sampling* of women for its proper testing).
iii) Some of the "functional additions" are significant, those for *feeling* and to a lesser extent for *thinking* being demonstrably significant.

There can be no doubt that if further women are individually examined in the above way, similar facts will be indicated. Every now and then a "functional addition" which is significant for *sensation* or for *intuition* would be found. Proceeding along such lines, then, we could conclude that, out of every 100 women, perhaps something like 50 will regard themselves as introverted, and far fewer, say 10, as extroverted; of the former, most will be *feeling* in function, fewer thinking, still fewer intuitive, and fewer sensation. The remaining 40 will appear to be of neither type with respect to effect X. Similarly for the extrovertive person-type. The proportions for the "functions" might be, very approximately, for those which are significant, in the order 50:30:15:5 for every 100 women, for *feeling*, *thinking*, *intuition*, and *sensation*, respectively.

INTROVERSION-EXTROVERSION PERSON-TYPES

The second of the propositions listed earlier (p. 157) has already received an empirical test in the facts just reported for the fifteen women. But we have to illustrate some of our procedures, and therefore it is useful to put proposition No. 2 to a direct test. It asserted that introvert or extrovert types can be indicated for any small number of persons. By "types" we shall mean *factors* and corresponding interaction person-types. The possibility exists, therefore, of the occurrence of pure or mixed types.

We have available some data for a graduate class of students who gave self-descriptions in terms of an unstructured sample of 121 Jungian statements. These statements had been drawn from the much larger universe already mentioned.[1] It matters very little, for the immediate purpose, that the students knew beforehand that Jung's theory was at issue, because none had ever read Jung's book

TABLE 3

Score	10	9	8	7	6	5	4	3	2	1	0	
Frequency	3	5	8	13	19	25	19	13	8	5	3	($n=121$)

and none could give a coherent account of the theory. Each member of the class was asked to provide three descriptions, following the instructions outlined above for the case of the landlady, as follows:

(*I*) Describe an ideal introvert.
(*E*) Describe an ideal extrovert.
(*S*) Give a self-description, as habitually understood.

All made use of the frequency distribution in Table 3.

The order in which these three were assessed was randomized, so that some students did *a* first, some *b*, and others *c*, and this, we could show, had no effect upon the results. Fifty students made each of these three appraisals as a class exercise, and each also correlated his own three arrays, as we see was done for Mrs. X above. Thus if r_{IS} is positive we may suppose that the person regards himself or herself as introvertive, and if r_{ES} is positive, extrovert instead. The correlation r_{IE} was expected to be high and negative, as it turned out to be for all the individuals, as Table 4 indicates.

If we were to intercorrelate all 50 students, each for three arrays,

1. They are listed in a factor-array in the appendix to this chapter.

11,175 coefficients would be involved. But nothing like this number needs attention. Instead, only sections of the data need to be looked at. A few smaller sets of persons may be chosen, for example, to represent the others: 10 may be selected who regard themselves as introverts (according to their correlation with their own ideal introvert array), another 10 who consider themselves extroverts on the same criteria of correlation with their E-ideal. Or a few might be chosen at random. Another set may be taken, comprised of those who provide no significant correlations r_{IS} or r_{ES}. *In all Q-technique studies it is important to realize that sections of the data can be taken*

TABLE 4

FREQUENCY DISTRIBUTION OF CORRELATION COEFFICIENTS r_{IE}, FOR n = 121 STATEMENTS

$-r_{IE}$	No. of Students Providing It
0.95–1.00	1
.90–0.94	7
.85–0.89	14
.80–0.84	17
.75–0.79	7
0.70–0.74	4
Total	50

apart in this way for separate factor analysis. As always, any such smaller section can be duplicated.

For the immediate purpose the 50 persons were separated into three sets of 15, 15, and 20, on the basis of the correlations that each person provided between the *Self* and the I- or E-ideals. The first set of 15 contained 8 women and 7 men, each of whose S-assessments had correlated highly (not less than about 0.40) with his or her *ideal* assessments I or E. The 8 women's S-arrays correlated *positively* with their I-, and *negatively* with their E-arrays; the case was the other way around for the 7 men. The second and third sets contained the other cases chosen randomly into two groups, with 15 persons in the one and 20 in the other. We shall be concerned in more detail only with the first of these sets, but, except that the S-correlations are *lower*, the facts for the other sets are similar to those we are now to consider.

Table 5 gives the correlation coefficients between the 15 persons, for their S-assessments. We notice that, although almost all the

coefficients are positive, those between the 8 women (A to H) average above 0.40, those between the 7 men (I to O) likewise are high, but the "cross"-correlations, between the men and the women, are low. A Thurstone or Burt centroid analysis of the table provides two factors, I and II, with the loadings shown in Table 6. On rotation

TABLE 5*

CORRELATIONS BETWEEN 15 STUDENTS FOR $n = 121$ JUNGIAN STATEMENTS ASSESSED FOR SELF-APPRAISAL (S). EIGHT ARE WOMEN (A TO H) AND SEVEN ARE MEN (I TO O)

	A	B	C	D	E	F	G	H	I	J	K	L	M	N	O
A	—	387	459	577	376	352	264	503	088	096	085	030	−005	023	030
B		—	603	493	242	431	307	392	−017	174	114	−006	049	082	159
C			—	615	463	508	399	522	−006	105	095	−043	080	094	099
D				—	398	572	441	607	087	083	138	062	100	172	063
E					—	557	320	500	137	190	148	107	077	140	172
F						—	324	467	095	089	128	013	044	015	101
G							—	370	−250	090	−040	−015	007	108	084
H								—	073	055	003	060	103	081	105
I									—	357	365	251	208	197	324
J										—	392	409	483	392	580
K											—	380	390	275	396
L												—	353	333	400
M													—	375	428
N														—	389
O															—

* Decimal points are omitted in all cases.

TABLE 6*

FACTOR SATURATION FOR TABLE 5

	I	II	I′	II′
A	.46	.38	.60	.00
B	.48	.33	.58	.06
C	.57	.46	.73	.02
D	.63	.46	.78	.06
E	.56	.25	.59	.17
F	.52	.41	.66	.02
G	.37	.36	.52	−.04
H	.54	.43	.70	.02
I	.30	−.36	.00	.47
J	.49	−.44	.08	.65
K	.42	−.40	.07	.58
L	.32	−.44	−.04	.55
M	.37	−.44	.00	.58
N	.36	−.33	.06	.49
O	.47	−.46	.06	.66

* $n = 121$ Jungian statements assessed for self-appraisal by 15 students, 8 women (A to H) and 7 men (I to O). I and II are centroid unrotated factors; I′ and II′ are the same rotated approximately 45°.

these can be encompassed almost completely within a positive manifold, with the loadings shown at I′ and II′, respectively, in Table 6.

We may say, therefore, that the women are of a person-type, I′, and the men of a person-type II′, the two being orthogonal; moreover, simple structure is involved, for the women have little or no significant saturation with factor II′, and the men have none significantly with factor I′. The women's factor is rather more clearly defined than the men's, with slightly higher loadings. It is perhaps of interest to see the plot for these factors: Figure 1 gives it for factors I and II, and also for the new axes I′ and II′.

We can add to this the loadings obtained from each of the other two tables, for the 15 and the 20 students, respectively: this assumes that the factors in the three tables are *invariant*, and this is approximately the case. Figure 2 shows the plot, then, for all 50 students, including the 15 shown in Figure 1. The axes I′ and II′ clearly serve reasonably well for all the 50 cases.

Here, then, is quite concrete evidence for two main factors. The one for the women represents an introversion conception that each has about herself (as can be seen from inspection of their arrays—but the precise nature of the factor is to be given later), and that for the men an extroversion conception that each man has about himself. The two are not *negatively* correlated, however: they are not correlated at all.

This may be regarded as a prototype of any studies of the kind that will involve person-typification. Whether orthogonal factors or correlated factors will be found or be acceptable is a factual matter. It happens in this case not only that the factors are orthogonal but that for the clearly delineated persons in Table 6 (A to O) they also represent simple structure, no person involving himself or herself in *both* factors I′ and II′. We hope that no one will retort, "But these only represent what the students *think* they are like, not what they might *really* be"; this is a matter we shall look at later. And if it is said, "But surely the students had no alternative, since the statements involved no other possibilities," then again we must notice that nearly half the persons gave merely chance results, i.e., appeared to be significantly neither type I′ nor type II′. The other half apparently had at least this choice before them, too. But we shall also see that these factors I′ and II′ encompass only about half the

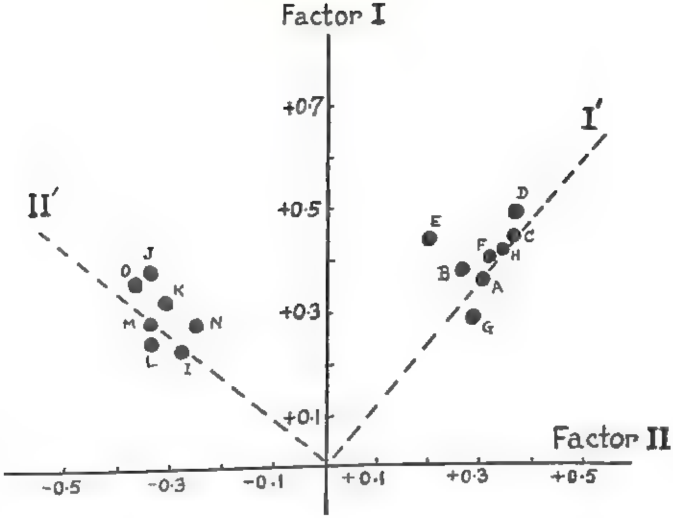

FIG. 1.—Showing factor loadings plotted, for data in Table 5

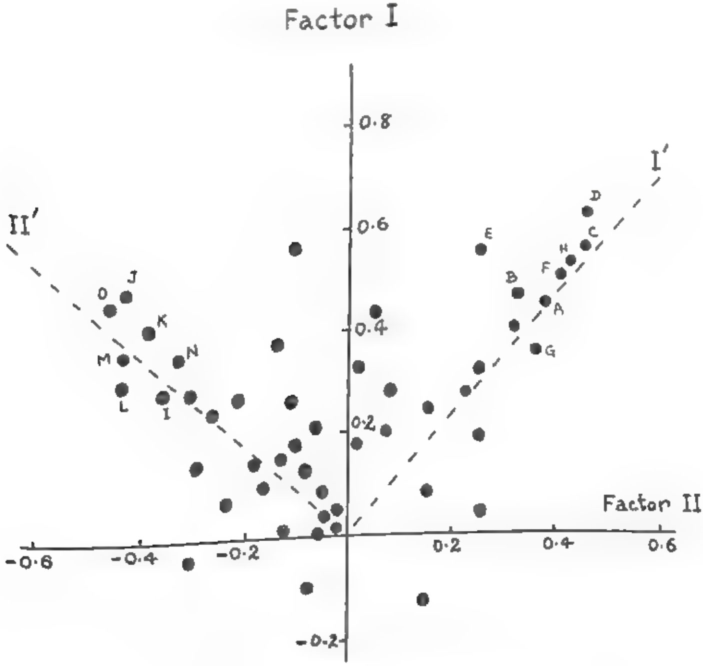

FIG. 2.—Showing factor loadings for all 50 students

available *reliability-communality* for most of the persons, so that, in addition to these factor results, there is much more to look at, namely, each person's *specificity*. Since each specificity, as the name suggests, is peculiar to each person, we see that there cannot be much in the aforementioned argument that only *two* possibilities confronted the persons: on the contrary, at least 52 such possibilities were there, apparently! And, indeed, there were many more. We might add, for the sake of simplicity, that no reference has been made to a few minor factors dotted about the three tables. Thus, even in Table 5 the variables G and I are particularly (negatively) correlated, and a third factor, III, would be necessary to represent the fact. But these are details which in no way impugn or otherwise delimit this outline. Far from it, for it so happens that G and I are husband and wife, a detail of some interest. Incidentally, the facts of *specificity* can provide a test of proposition No. 4 of our list (see p. 157).

THE FREQUENCY DISTRIBUTION OF PERSON-TYPES

The problem listed as No. 10 on page 158 inquired about the shapes of the frequency distributions for *person-types*. These are obviously empirical matters. If several hundred persons are tested along the above lines and their type saturations calculated[2] as for factors I′ and II′, four frequency distributions would be required to cover the facts. It happens that the I′ factor is slightly different for men and women, and likewise for factor II′. Women extroverts, it seems, are not altogether the same as men extroverts, and men introverts are rather different from women introverts. But, having separated persons into their respective types and having calculated the factor loading of each in the type or types to which he or she belongs, it is a simple matter to draw up the required distributions. It is difficult to see what essential interest they will have for anyone, and therefore it is not proposed that any such study should be made. It is sufficient to hazard a guess that each of the four distributions, for men with respect to factors I′ and II′ and for women for the comparable factors, will be roughly (*a*) hyperbolic or (*b*) modal, as

2. When large numbers of persons are involved, it would be sufficient, theoretically, to determine each person's factor saturations by correlating his or her array with the factor-arrays for the respective factors.

shown in Figure 3. This is very different indeed from the distributions usually discussed for introversion-extroversion in textbooks.

THE SPECIFICITIES

The person-types so far discussed in factor (correlational) analysis refer to *communalities* only, that is, to correlations that two or more persons have in common. It is possible to go further in terms of the factor theorem which relates a person's *reliability* coefficient

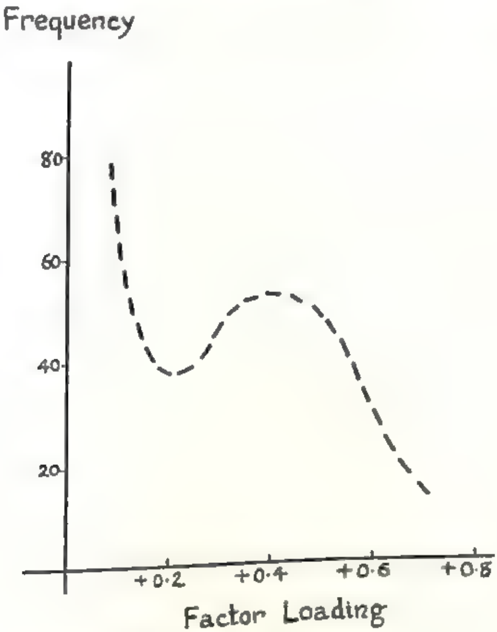

FIG. 3

in part to his communality factors and in part to a factor or factors which are *specific* to himself. Component analysis is in terms of communalities only; but originally, in Spearman's analysis, a considerable to-do was made about specific factors.

Thus, given a person's reliability coefficient, r_{AA}, and his factor loadings in factors such as α and β, we have

$$r_{AA} = r^2_{A\alpha} + r^2_{A\beta} + r^2_{AS} \ldots \quad \text{(see 155)}.$$

Here A is the person variate and S the specificity factor, and $r^2_{A\alpha} + r^2_{A\beta}$ is the person's communality.

In the tables of correlations so far discussed, no reliability coefficients were referred to. But Mrs. X correlated 0.82 ± 0.02 P.E. for her two self-appraisals, and this is about the size obtained for most persons. Even if a reliability coefficient of only 0.80 is assumed for each of the 15 persons in Table 5—and few would be much lower than this in amount—the respective sizes of the communality factors (I′ and II′) and each person's *specificity* are brought clearly into focus in Table 7.

The specificities are everywhere about as high as the communality factor loadings. If allowance is made for a number of minor communality factors, such as one between G and I referred to earlier, there still remains as much variance centered about each person specifically as there is in the communal factors I′ and II′; the totals

TABLE 7*

Person	A	B	C	D	E	F	G	H	I	J	K	L	M	N	O	Total
I′	60	58	73	78	59	66	52	70	00	09	07	−04	00	06	06	5.40
II′	00	06	02	06	17	02	−04	02	47	65	58	55	58	49	66	4.29
Specificity	66	68	52	44	65	60	73	56	76	61	68	71	68	75	60	9.59

* Decimal points are omitted.

are indeed almost equal as they stand (9.59 for *S*-factors, 9.69 for factors I′ and II′). In all Q-studies it is highly important, more than it was in R-technique, to be cognizant of the specificities.

FACTOR-ARRAYS

Factor-arrays consist of all the statements or the like of a Q-technique sample, arrayed in rank order of their factor scores. The statement which gains the highest score for a factor is placed at the head of the list and that scoring least is placed at the bottom. In this way all the statements are laid out before us; we can then look them over, much as we might look down a list of the names of students who have been ranked in order of their achievement at school. It is these arrays, of course, to which variance analysis is applied when a structured sample is involved.

In R-technique, each *person* can be given a score, in standard terms, for his ability in any factor: this is his factor score. Thus, if

the factor is g, for a test t whose factor loading is r_{tg}, the factor score g_a for a person A is the following estimate (148):

$$g_a = r_{tg} \cdot x_a \pm \sqrt{1 - r_{tg}^2}, \tag{i}$$

in which x_a is the score gained by the person A in the test t, in standard terms. When several tests are involved, all of which have loading in the factor g, and only g, the best estimate of A's factor score is given, of course, by a weighted average of his factor scores on the several tests; and Spearman (148) has shown that, for the best estimate, the "weights" should be proportional to

$$\frac{r_{tg}}{(1 - r_{tg}^2)}. \tag{ii}$$

It is of course a simple matter to arrange statements of a sample in Q-technique in the order of their factor scores, the expression (i) applying here, as in R-technique, except that t will now represent a *person* and x_a the score gained by a *statement A* when it is operated on or with respect to the person t. The factor would be peculiar to Q-technique. The best estimate of the statement's factor score can be gained, as in R-technique, by "weighting" its scores in several person variates, and again precisely the same expression applies for the best "weights," namely, (ii) above. The factor scores for each of the statements of a sample are so calculated, after which the statements are arranged in rank order, and this is the factor-array. Precisely the same procedure is followed for a *type*-array, as we shall indicate later.

As an example of the calculations involved, we take a small table of correlations for five persons who assessed themselves with an unstructured sample of 121 Jungian statements, the same as those employed for the study on introversion-extroversion types above. The correlation table for the five women is given below (Table 8). Four of these subjects appear as persons A, B, C, and E of Table 5 above. The women's self-appraisals (S) are at issue, and in each case a reliability coefficient was obtained by repeating the self-appraisals a day or so later.

One factor is sufficient to subsume this little table, a Spearman analysis providing the loadings a. These are not identical with the loadings for the same persons A, B, C, and E of Table 5, largely

because they are based, of course, upon a small matrix; but they are approximately invariant, and, in any case, they will serve our exemplificatory purposes. The specificities are shown, and again these are high, except for person C.

We have to calculate the factor-array for factor a by taking the best-weighted sum or average for every statement of the sample in turn, for the five persons, all of whom are loaded with factor a and with this factor only.

There are 121 statements, numbered for convenience from 1 to 121, and five women, each of whom arrayed these statements on the basis of the frequency distribution given earlier (p. 167). The scores

TABLE 8*

SHOWING CORRELATIONS BETWEEN FIVE WOMEN WHO REGARD THEMSELVES AS INTROVERTIVE FOR THEIR SELF-APPRAISALS
($n = 121$ Jungian Statements)

	Persons					Factor a	Specificity
	A	B	C	R	E		
A	(784)†	387	459	282	376	55	69
B		(884)	603	320	242	57	75
C			(836)	594	463	89	23
R				(835)	526	65	64
E					(796)	60	66

* Decimal points are omitted.
† Reliability coefficients are in parentheses.

provided originally by the women will be laid out in some such form as in Table 9. All five arrays have the same mean and the same standard deviation. If we regarded the five women as of equal "weight" with respect to factor a, it would be sufficient to add the five scores for each statement as they stand; the 121 totals would be the required estimated factor a-array. But the persons should be "weighted" so as to provide the "best-weighted pool" (155), that is, an array which will have the best possible approximation to the factor a, and it is for this that the "weights" of expression (ii) above are required. If two persons have loadings p and q, respectively, in factor a, the "weights" would be in proportion to each other, as follows:

$$\frac{w_p}{w_q} = \frac{r_{pa}(1 - r_{qa}^2)}{r_{qa}(1 - r_{pa}^2)}. \qquad \text{(iii)}$$

Here r_{pa} and r_{qa} are the factor loadings of the persons P and Q in factor a, and the required "weights" are w_p and w_q.

For Table 8 the "weights" are given in Table 10, expressed in terms of A, who has the lowest loading in the factor. Person C has to be given five times as much "weight" as A or B, and four times as much as R and E. For simplicity, the weights are assumed to be 1, 1, 5, 1.5, and 1 for the five women, respectively.

TABLE 9

Statements	Scores Provided by Persons				
	A	B	C	R	E
1	8	3	9	10	10
2	5	3	4	6	5
3	10	10	10	7	9
4	6	7	7	4	5
5	7	9	10	8	7
6	2	5	5	6	9
.	.	.	.	.	.
.	.	.	.	.	.
.	.	.	.	.	.
121	5	4	6	8	6
Mean score	5	5	5	5	5
Standard deviation	2.3	2.3	2.3	2.3	2.3

TABLE 10

Person	a	w	Approximate w
A	0.55	1.00	1
B	0.67	1.08	1
C	0.89	5.17	5
R	0.65	1.44	1.5
E	0.60	1.20	1

These "weights" are now applied to the scores for each of the 121 statements in turn: the scores for person C are multiplied by 5, and those for R by 1.5. The scores per statement are then added, as shown in Table 11.

The final array is the best estimate we can make of the factor a. The scores as such can be transformed, of course, either to standard scores or to another array with a mean of 5.0 and standard deviation of 2.30, the values of the original arrays.

Somewhere we have to place on record a complete factor-array, and we do so in Appendix I for this factor α, calculated as above for the women A, B, C, E, and R. The weighted scores have been reduced to the values employed in the initial distributions, that is, scores ranging from 0 to 10, with the frequencies given above (p. 167). The statements have then been ranked in order, from those gaining most to those gaining least scores for the factor. So arrayed, the factor-array represents what a person would provide who is more highly saturated with the factor α than any of these five women, and such that these women will correlate with it by the

TABLE 11

Statements	Weighted Scores for Persons					Weighted Totals
	A	B	C	R	E	
1	8	3	45	15	10	81
2	5	3	20	9	5	42
3	10	10	50	10.5	9	89.5
4	6	7	35	6	5	59
5	7	9	50	12	7	85
.	.	.	.	.	.	.
.	.	.	.	.	.	.
.	.	.	.	.	.	.
121	5	4	30	12	8	59

amount of their respective loadings in the factor. Clearly, this factor-array could be provided by a person, in actual operations, who happens to be more highly saturated with the factor than any of these five women. In this sense it is by no means merely hypothetical but is itself a testable proposition.

There will be a factor-array of this kind for every factor of an analysis in Q-technique. Had the correlation table (Table 8) brought to light another factor, β, a factor-array would be calculated for it, composed of these same 121 statements but in a different order, uncorrelated with that for factor α.

Factors in Q-technique may indicate "pure" types, as defined earlier (p. 108), or "mixed" ones when more than one factor is involved. The above factor-array would also be the "pure" type-array for factor α. If there are two or more factors β, γ, and persons X, Y, Z, . . . , have each of them in similar proportions, a type-array can

be calculated for the "mixed" type in the same way as for the pure type.

If the reader cares to read down the factor-array for a in Appendix I, it will probably occur to him that it represents a stereotypy. It is what university women would perhaps like to think they are rather than what they really are. To this extent, proposition No. 3 of our earlier list (p. 157) is involved.

PREDICTIVE FACTORS

Factor-arrays, of course, are just the factors themselves laid out before us in detail. They can be placed on permanent record, just as has been done for factor a above, and they are then available for scientific purposes other than the one to which attention has just been given. Others can always look at a factor-array themselves and explain it as they may wish, in terms of their own theoretical interests. Or the variate may be added as a "reference value" to other experimental studies, for comparison with the conditions under which the factor was reached in the first place. Thus we might wonder whether a few emancipated young women from Radcliffe are subject to the same stereotypy as the gentlewomanly students of Chicago, and the matter can be tested by asking the Radcliffe women, A, B, C, D, . . . , to give self-descriptions in terms of the 121 Jungian statements: to N such variates we could add that for factor a, making $(N + 1)$ variates in all for factor analysis. The analysis would show whether or not the women A, B, C, D, . . . , were loaded with factor a or whether quite a different factor is at issue. But the experimentally orientated psychologist will see at once that we can go much farther in this direction: it is possible to compose a factor-array quite artificially, on hypothetical grounds, and to correlate it with a matrix of experimental variates, to see whether the latter can receive an explanation in terms of the former. In general, indeed, we can now artificially compose a whole set of variates, to represent hypothetical conditions, correlate and factor them, and then perform an experiment for the variates, to see whether or not the expected results make their empirical appearances. In this way the results expected from an experiment can be predicted, the experiments can be performed, and a test can be made at once of the correctness of the predictions—all in factor terms.

Another inference may be mentioned, in relation to Jung's concept of an *ideal* introvert or extrovert, which we did not list among the propositions on page 157. Each of the 50 students referred to earlier gave a description of what he or she believed an *ideal* introvert, and an *ideal* extrovert, to be. Any table of correlations for these ideal descriptions is strongly bipolar, with high loadings at issue, there being only one factor, say γ, positive loadings being ideal introvert, and negative loadings ideal extrovert. That is, the students have a very definite conception of these terms in a stereotypical manner. We assert, therefore, that much the same factor γ can be reached from *any* university students in America. We regard this as a valid generalizing inference, because our knowledge about stereotypy and about the widespread ostensible learning that has taken place with respect to the terms "introvert" and "extrovert" makes the *fact* of factor γ a concrete operation. If it occurs among 10 or 20 or 50 persons under these operational conditions, it must tend to occur for any others of a similar kind. The factor α for the 5 women is operationally defined for these 5 only, but it, too, could well be approximately invariant. Or we could add a few more women to the 5, to make a better estimate. The law of diminishing returns soon applies, however, and as few as 10 or 20 persons may be quite sufficient to define a factor for which invariance can be asserted with considerable credibility.

But we doubt whether the invariance in question is important or a matter for serious scientific concern. Factors, even of this kind, must reflect, to some degree, the personalities of the women themselves in cultural subgroups or the like. Highly predictable factors are quite easy to provide, and the notion that "large numbers of cases" are essential to valid scientific inferences is again seen to be the dogma that it very often is.

COMPARATIVE STUDY OF SAMPLES FROM DIFFERENT UNIVERSES

Some of the most important applications in Q-technique involve the use of two or more samples from different universes in the course of studying the same few persons. Thus ten women might be appraised with respect to (i) a Jungian set of statements, (ii) a set of statements structured in terms of psychoanalytical theory, (iii) a

sample of statements from R. B. Cattell's work on temperamental qualities, and (iv) a set drawn from H. A. Murray's study of personality. Each sample is likely to provide its own factors, and, in general, one has to look for relationships between these in terms of how they are distributed among the experimental persons. In the simplest case one would ask whether two or more such samples "pick" upon the same persons for their respective person-types, pure or mixed. Or, in general, we could study relationships between factors, and their loadings, for different samples, by way of contingency tables, χ^2 tests, and the like.

TABLE 12

Persons	Sample I Factors			Types	Sample II Factors			Types
	A	*B*	*C*		*a*	*b*	*c*	
P_1	*X*	0	*X*	*AC*	*X*	0	0	*a*
P_2	*X*	0	*X*	*AC*	*X*	0	0	*a*
P_3	0	*X*	0	*B*	0	*X*	*X*	*bc*
P_4	*X*	*X*	0	*AB*	*X*	*X*		*ab*
P_5	*X*	*X*	0	*AB*	*X*	*X*		*ab*
P_6	*X*	0	0	*A*	*X*	0	*X*	*ac*
P_7	0	*X*	0	*B*	0	*X*	*X*	*bc*
P_8	0	*X*	*X*	*BC*	0	*X*	0	*b*

Thus, consider samples I, II, III, . . . , drawn from different universes, such as i–iv above. Let there be experimental persons P_1, P_2, P_3, ... As a result of their respective operations, a factor composition will be determined for each of these persons for each sample. Thus, for sample I the factors may be *A*, *B*, and *C*, and these may occur for eight persons P_1–P_8, as shown in Table 12. These permit us to classify the persons as shown, P_1P_2 being alike, P_4P_5 and P_3P_7 each being alike. For another sample II the factors may be *a*, *b*, and *c*. These, however, may permit us to classify the persons in the same way, P_1P_2 again being alike, P_4P_5 and P_3P_7 likewise. Comparative studies of this kind can be the basis for studies of *functional autonomy* and the like (174).

In this case three factors are involved, but they allow us to classify the same persons into types—pure, mixed, or none. This can be achieved in a large number of different ways—it is even

possible if there is only *one* factor for a particular sample, since quantitative differences in factor loadings are also a basis for classifying persons. If perfect contingency of the kind we are discussing exists, then we might reasonably expect that common influences are at work in terms of the different samples. Either the explanation or the "interpretation" that we can give to the factors for the one sample is the same that we can give to the others for the other samples, or some causal agency must be assumed. The whole question of the comparative study of universes, for the same persons operating upon their samples, is clearly one that offers scope for much development in the future.

FURTHER PROBLEMS

Most of the propositions listed on pages 157–58 await experimental attention. Some require detailed investigation, and we can say only a few words about them here. Our main thesis, it will be remembered, is that a theory should be worked at by way of singular testable propositions. Thus, instead of trying to measure introversion-extroversion, as in R-methodology, we set about designing experiments to put these various propositions to test under singular conditions. The designs are likely to be as individual as are the problems or propositions themselves. Thus proposition No. 9 has to do with the possibility that "perseveration," so-called, is related to introversion-extroversion. Spearman (148) believed that a universal principle could be found which would account for all personality types, including those adumbrated by Jung, the primary and secondary functions of Gross, Heymans, and Wiersma of the Dutch school, the analytical and synthetic types of the Külpe school, the perseveration types of Müller and Pilzecker, and many more besides. All were to be subsumed, according to Spearman, by a factor (p) of perseveration. We can attach little that is credible to such a factor. Yet it is a simple matter to test what is probably true, namely, that descriptions of persons in Jungian language correspond to those given in the earlier literature in terms of so-called "perseverators" and "nonperseverators." The experimental design consists merely of constructing two samples of statements, one, Q_1, for the Jungian theory (such as we have used above), and another, Q_2, for the theory of perseveration. Any N persons (usually 10 or so) are

then used as experimental subjects. Each provides a Q-sort self-description with Q_1 and also with Q_2. Two $N \times N$ correlation tables result, one for the Q_1 sample and the other for Q_2. The testable proposition at issue is then simply whether the *factor patterns* in the two cases are comparable. That is, if persons X, Y, and Z have the same factors for Q_1, are they also alike with respect to Q_2? The proposition was confirmed in a study made along such lines by three of our students. This suggests that tangible facts are at issue. But we are not obliged to interpret them as due to either perseveration or introversion-extroversion.

Similarly for each of the other propositions. Number 6, for example (p. 158), asserts that introverts can really see into the "heart" of a fellow-introvert and likewise that extroverts have special insights into fellow-extroverts. Again we have little doubt that Jung has based his conclusion on some tangible, concrete facts. The design for a study to test this proposition has been reported elsewhere (174), in one of our earlier studies. Students were called upon to assess themselves on a sample of Jungian statements along Q-lines, and then severally assessed their lecturer. The small class of students agreed that the lecturer was extrovertive in type, as did the lecturer himself. It was shown, however, that the extrovert members of the class correlated specifically with the lecturer: that is, they emphasized some statements which were of particular significance to themselves and also to the lecturer, whereas the introverts did not do so.

With respect to the "individuation" problem No. 5, it is a simple matter to apply Q-technique approaches to the study of a single family, including father, mother, brothers, and sisters. If we invite all to describe *themselves*, as well as their *ideal* personality or person, the role of *identification* can be studied at the same time.

Problem No. 7 has to do with "compensation." There are persons who are apparently really extroverts but who assume the opposite attitude for defensive or other reasons, much as Origen castrated himself in order to insure the stoic abnegations of an introvert. There is plenty of evidence that such behavior occurs. The introvert type I in Table 6 above (p. 169) consists in part of a superficial mask that women students wear in the interactional setting that comprises themselves as students and their instructor. But if we are so minded, these façades can be removed, either by way of a sym-

pathetic interview situation in which heart-to-heart confessions are fostered, or if we call upon the individual to make many probes into herself, in the manner described elsewhere (p. 256), sooner or later the person will "give herself away." If we had a more thoroughly defended Origen it would still not be difficult, we feel sure, to make him display his suppressed extroversion in some way or other, and if one were obsessional enough to wish to inflict a Q-sorting experiment upon him, the facts could be made to speak for themselves in factorial analysis or the like.

Problem No. 8 concerns the role of fantasy as a "bridge" between a person's claims of introversion and extroversion. The shy introvert has vastly daring escapades in fantasy; the restless extrovert sleeps on the bosom of mother nature in rural and benign seclusion in his daydreams. Something of the kind is suggested when we ask our subjects to give, *first*, a self-description along Q-lines, in terms of a set of Jungian statements, and, *second*, to describe (i) what sort of personality they admire most or (ii) what they themselves would have liked to be like or (iii) what is an ideal personality or (iv) what is the most sensible kind of personality to have, all in terms of the same set of Jungian statements. For now the introvert usually seems to veer toward more extroverted forms of preference in terms of i, ii, and iv, but not iii. They can idealize something of their own kind, iii, but can "feel themselves" into much more extrovertive behavior as a sort of near-fantasy expression of their wishes. In any case all these are now operational possibilities. But if we wish, instead, to handle a person X's own fantasy, this, too, is not difficult. Take him, put him through a Thematic Apperception Test suitable for the purpose in hand, and obtain his responses; these could represent his fantasy, and this can be dealt with along Q-technique lines, as we shall see in chapter xiv, which is devoted to this very matter. We might show, again, that the fantasy of an introvert X and an extrovert Y (the two types being identified in terms of a Jungian set of statements), when operated upon in Q-technique terms (i) by experts or (ii) by a number of similar introvertive persons, on the one hand, and a number of extrovertive ones, on the other, bring to light the compensatory "bridges" of the kind Jung had noticed in clinical situations and in cultural patterns in general. We believe that one could research for a long time on fantasy and bring many different experi-

mental approaches to bear upon it, along lines that the various sections in this book illustrate or exemplify.

Another of Jung's propositions, not listed earlier, was to the effect that an extrovert has a certain "repugnance, fear or silent scorn" for introversion, and *pari passu* an introvert for extroversion. This seems to contradict what has just been said about No. 8 above. For it has been suggested that an introvertive personality may tend to accept and even extol or admire extroversive forms of behavior in fantasy, whereas this proposition states the opposite. How can they be reconciled? The answer in Q-technique terms is very simple, and it illustrates a most interesting aspect of the technique: *the facts in No. 8 would be looked for in one sample of statements, and those for "silent scorn" in quite a different sample.* In the first case the statements would be such as the introvert can consciously acquiesce in and accept, whereas the statements required for the latter would touch on deeper matters, with more unconscious factors at issue that the introvert can no longer tolerate, even in fantasy. We have no doubt that the art of experimenting lies in the construction of these different "samples" to serve different purposes.

CONCLUSION

It seems, then, that the older type-psychologists were correct about types, *methodologically* regarded. There can be no doubt that the conception of types as extremes of normal distributions in no way represented what the Jungs, Sprangers, and Kretschmers had in mind, but were merely logical entailments of postulates in R-methodology. This is not to say, however, that the psychological involvement of any of the type theories is also correct. Theories are neither proved by verifying a few singular propositions nor disproved by finding that some cannot be verified. Nor, if we could prove thousands of different propositions to our own statistical satisfaction, is a theory necessarily acceptable on that account: there might be a better theory for the same facts. It is probably true that all the propositions about Jung's theory and a score of others can be satisfactorily proved along experimental lines of the kind we are discussing (for Jung was a keen enough observer of human behavior); but we might reject the theory of introversion-extroversion in favor, for example, of one relating the matters at issue to psycho-

analytical doctrine. The experimenter, however, either is faced with the necessity of *disproving* propositions by finding neat variate designs for the purpose or has to take a decision on his part to accept this rather than *another* theory, because of some wider possibilities he may envisage for it. The proof of a theory otherwise is the usual one of pursuing the rules of scientific procedure: the more propositions one can test and incorporate into the body of the theory, the more credible will a theory become, up to a point.

The method of exploring a theory, such as Jung's, has been roughly indicated. It consists of asserting propositions, designing experiments to test them under singular conditions, and patiently coming to conclusions in terms of facts. We have given only bare outlines of the kinds of experimentation required or envisaged. In general, we may suppose that these earlier psychologists really observed facts which we can bring under experimental control along Q-technique lines. We have never found this difficult to achieve. That facts of some kind occur for all the propositions listed about Jung's theory is certain, as our experiments prove—it is quite impossible to report all that we have done in this connection. But the explanation of these facts is quite another matter. We are not obliged to accept Jung's explanations. On the contrary, it is necessary to theorize quite differently. Many of the facts, indeed, are relatively superficial; the really significant theory is that which points to valuable propositions and to the discovery of unsuspected processes.

APPENDIX I

Example of a Factor-Array

121 Jungian Statements Arranged in Order for Factor a from Those Most Characteristic to Those Least, for Self-appraisals by Five Women A, B, C, R, and E

Score	No.	Statement
10	5	Feelings are genuine
10	3	Keeps her feelings to herself
10	18	Has quiet manners
9	1	Liable to be hesitative
9	12	Somewhat awkward in personal relations
9	13	Never supremely confident
9	35	Apt to be self-effacing
9	98	Silent, inaccessible, hard to understand
8	19	Her pleasures do not change from hour to hour
8	59	Shrinks from making herself or her opinion effective
8	86	Emotionally subdued
8	20	Her judgment is mild and lenient
8	107	Coldly reserved, but with intense feelings underneath
8	68	Tolerant
8	34	Apt to hide her personality
8	71	"Sensitive"
7	9	Occasionally sarcastic, censorious
7	23	Fits into existing conditions with relative ease
7	105	"Still waters run deep"
7	7	Calm and intelligent
7	11	Taciturn, shy, impenetrable
7	39	Not influenced by prejudice
7	104	"Sympathetic," brings insight and experience to bear on problems of others
7	28	Is concerned about the general welfare
7	92	Diffuses an atmosphere of repose
7	4	Consciously altruistic
7	31	"Contemplative"
7	120	Reasonable and just
7	60	Not difficult to please
6	90	Works slowly and with difficulty
6	42	Apt to be negative and depreciatory
6	114	Fatiguable; says she is "exhausted"
6	100	Is "reasonable"
6	101	Appears prickly, inaccessible, haughty
6	67	Somewhat enigmatic and egotistical
6	91	Outward appearance rather gauche; or unconcerned and naïve
6	65	Benevolently neutral, sympathetic in judgment
6	121	Kindly, generous, and hospitable
6	109	A victim of the ambition of others; tends to be abused
6	21	Laconic
6	116	Apt to judge neighbors and associates
6	53	Reasonable and sedative
6	103	Dresses well
6	93	"Her ideal is the actual and not a world of ideas"
6	14	Unconventional
6	25	Thoroughly realistic
6	32	A "practical idealist"
6	69	Compulsive

Score	No.	Statement
5	82	Rarely puts forth new ideas or opens new paths
5	118	Has no convictions, but also no misgivings
5	113	Mental outlook lacks range and depth
5	110	Fitful and restless
5	65	In thinking, intensity is her aim, not extensity
5	95	A little mischievous, cruel
5	92	Unhappy in repose
5	87	Rather maladroit, usually achieving the effects of what she set out to do
5	102	Especially prone to do what she is not asked to do
5	17	Judgment often at fault
5	70	Curiously inert
5	6	Gives an impression of being slow
5	38	Seizes hold of new notions eagerly but abandons them as soon as she has exposed their possibilities
5	30	Things and persons seize and rivet her attention
5	41	Is reflective, but sees crooked
5	22	Faultfinding
5	66	Affected, but not imitative
5	115	Enthusiasms are not sustained
5	57	"A voice crying in the wilderness"
5	112	"Can't think what she doesn't feel"
5	88	A dreamer, mystic
5	77	Fond of movement and change
5	72	Uses highly *subjective* language
5	119	Has frequent alterations of her ego or mood
5	79	Quick and opportune rather than persistent or consistent
4	48	An initiator, enterprising
4	76	Has a keen nose for things pregnant with future promise
4	36	A fruitless kind of personality
4	16	Enjoys life fo the full, without being gross or sensual
4	2	Full of life and activities
4	75	Self-willed and inflexible
4	27	Apt to be resentful
4	40	Spontaneously appreciates, congratulates, and praises
4	106	Invaluable in social movements
4	54	Feelings are petty, cross-grained
4	10	Petulant, fussy
4	73	Implacable
4	84	Slow to forgive
4	99	Rigid and unbending in opinions
4	61	Apt to be disproportionately and easily irritated
4	33	Fitful and uncertain in temper
4	83	Her judgments appear to be cold, arbitrary, obstinate, and inconsiderate
4	63	Inspires others with ideas, etc., which she animates and embodies
4	29	Praises others heartily
3	74	Apt to follow the guiding line of her feelings
3	63	Apt to be a crank
3	85	Uneasily covetous
3	117	Full of projects and bustling activity
3	51	Puts her whole life into situations or ideas, ruthlessly so

Score	No.	Statement
3	108	Dogmatic
3	50	"Lacks the true human note"
3	73	Lacking in self-criticism
3	56	Apt to be sullen and given to bursts of anger
3	49	Delights to entertain and be entertained
3	80	Charming and lively capacity for enjoyment, a jolly fellow
3	94	Life occupied with little things
3	89	Natural tendency to be interested in historical or accepted forms of belief and policy; conservative
2	96	Thinking is so difficult—therefore she prefers not to pass judgments
2	111	"Nagging"
2	8	Open and sociable
2	48	Fond of administration
2	37	Disturbing and provocative
2	47	Tactless and unsparing
2	81	Becomes entangled in scruples
2	45	Gives an impression of pose, inconstancy, and unreliability
1	97	Passions and emotions are so strong that they hold reason in subjection
1	15	Her enthusiasm spills over into the surroundings.
1	44	Alert, to the point, witty
1	64	Superficial, shallow, almost spurious
1	58	Passionate in affects
0	24	Impulsively demonstrative
0	26	"Ready to command a fleet or to amputate a leg"
0	43	Troublesome, even to some violence in thinking and passion

CHAPTER IX

THE PRIOR ANALYSIS OF QUESTIONNAIRES

QUESTIONNAIRES

INVENTORIES and questionnaires serve two main purposes. They may provide a basis for *counting* certain facts—how many students are married, for example, and how many single can be determined by counting heads—for large-scale and representative sampling conditions. But questionnaires are also widely used for making broad surveys over diffuse fields of study, "to see what goes with what," with the hope of discovering less obvious facts about a situation. A questionnaire is usually composed with some kind of theoretical standpoint or expectancies in mind, in however rough or implicit a fashion. It may be suspected, for example, that a higher proportion of married than of unmarried students is properly motivated for college life. Asking a large sample of students whether they are properly motivated or not will scarcely elicit this information. Instead, it has to be *inferred*, perhaps from relationships between various responses to apparently unrelated questions on a questionnaire. In general, the less obvious or more theoretical issues for a questionnaire are of this nature. If, however, means can be found for *operationally* defining what is meant by "proper motivation" (to continue this example), it would be a simple matter to count its incidence, precisely as one counts how many students are married or unmarried. Our present concern is with the possibility of such operational definitions for data ordinarily dealt with by questionnaires. We are to propose that along Q-technique lines it is often possible to discover complex facts, of the kind usually regarded as *inferences*, by previous study of relatively few cases only. They can thereupon be counted, if need be, by using an appropriate questionnaire and large-sampling techniques. In short, we are to argue that it is often possible to analyze a questionnaire along Q-lines, to determine the intrinsic possibilities of reaching data of the inferential kind. The matter has wide ramifications, only a few of

which can be touched upon here. In particular, however, studies are common in the social sciences in which hundreds or thousands of cases are employed, when more might be learned from preliminary Q-studies on a few cases only.

AN EXAMPLE

The issues can be exemplified in terms of almost any questionnaire containing many questions. We have chosen one, however, on student opinions, with which we have had some firsthand experience: it is called the *General Questionnaire G-3*.[1] This had been applied during 1947 to many thousands of veteran students in colleges on a nation-wide basis, with the object of exploring what they had to say, among other things, about (*a*) their *motives* vis-à-vis college education and (*b*) their *habits of study*, or attitudes about study.

It is helpful to have the *General Questionnaire G-3* concretely before us: it contains over 200 questions, and its twelve pages cannot be reproduced here. The questions it contains, however, are of the following kind:

5-22. People go to College for a wide variety of reasons, and colleges usually try to satisfy these reasons. In the list below are reasons which many students have given when asked why they were going to college. After each statement indicate to what extent the reason is important to *you*. Encircle the number "1" if the reason does NOT APPLY or is of NO IMPORTANCE to you; "2" if it is of SLIGHT IMPORTANCE; "3" if it is of MODERATE IMPORTANCE; "4" if it is of CONSIDERABLE IMPORTANCE; and "5" if it is of GREAT IMPORTANCE.

5. College life will help me to develop socially.	1 2 3 4 5
6. Most of my friends are going to college.	1 2 3 4 5
7. A college degree is necessary for the kind of work I want to do.	1 2 3 4 5
8. I hope to make a great many new friends in college.	1 2 3 4 5
9. It has always been expected that I would go to college.	1 2 3 4 5
10. I hope to acquire some qualifications for community leadership.	1 2 3 4 5

. . . and so on.

The *General Questionnaire G-3* had been prepared initially with the notion, in part, that it might provide some additional grounds upon which to base predictions about the success or failure of students at college. It is well known that selective tests of the scholastic kind are not a complete solution to the problem of predicting

1. This is Form G-3 of an inventory prepared by the College Entrance Examination Board. Our studies of the questionnaire were conducted during the winter quarter of 1948 at the Educational Testing Service, Princeton, New Jersey. We wish to thank Dr. H. Chauncey for the opportunity to see the work of this important organization.

academic success or failure, although they are the best single indicators. Many students do well on *scholastic aptitude* predictors, yet fail at college—these are the so-called "underachievers." Others who do poorly at aptitude tests turn out to be quite excellent scholars—relative to the tests they may be called "overachievers." Every manner of reason can be put forward for these apparent disparities. One in particular suggests that scholastic tests do not tap the core of a student's motivation for college life. The psychologist therefore proceeds to devise means, if he can, for measuring *motivation*, much as he had done earlier for *aptitude*. In principle, that is, the attempt is to measure effective variables, such as aptitude and motivation, and to use these for predictive or other purposes.

It is at this point that we should stop to think for a moment. Each such measurement of an attitude or a motivation is made according to the "rule of the single variable." That is, the instruments are designed to measure one thing at a time, in principle, and everything is done to achieve this and to keep all other conditions constant. This "rule" was long regarded as essential to any sound experimental work, as Woodworth has so clearly noted in his *Experimental Psychology* (206). It is against it that Fisher has directed his attention (68). In mental testing, in our view, operation of this "rule" leads to the measurement of *potentialities* only. The result of such a rule has been to provide data which have potential significance, but not necessarily any other. Concrete behavior is more likely to occur in a setting in which many effects and variables mediate *together*, if at all. A student's everyday concern is with a complex behavioral situation, for example, in which his aptitudes have to scramble for a place among many other influences. Similarly, in Fisherian experimental methodology all the specified independent variables are brought to bear *together*, in the one concrete situation, and not according to the rule of the single variable. Thus what a person may *actually* do in a situation may bear little relation to what he is *potentially* capable of doing under propitious, that is, rule of the single variable, conditions. The hermit may have every capacity and use none.

Our own concern is to probe into actual situations rather than into potential ones only. When aptitudes, motivation, and the like are in functional relationship in a college situation, knowledge of a student's potentialities may not be pertinent to the concrete situation.

Nor is it the case that the procedure by which potentialities are separately measured is somehow correct, profound, or fundamental and that it alone can presume to be scientific. Nor, if we could discover how to deal with *actualities* rather than with isolated potentialities, would it be of any immediate concern to evaluate these procedures for their predictive value. Success at college may be predicted better by a test of potentialities than by any other means. Our interest is in the concrete behavior of a person, and we would prefer to look at this without undue preoccupation with analytical tools of the single-variable kind.

The questionnaire *G-3* was fashioned, we have no doubt, with the idea of reaching into the motives and study habits of students. No theory is discernible in its structure, however. Even so, we may ask what facts of an intrinsic kind it can hope to provide.

LARGE-SCALE TREATMENT OF QUESTIONNAIRE DATA

Before beginning the Q-technique studies, it is helpful to look at the methods which are in wide use for the statistical analysis of data from questionnaires. A fairly large number of questions are often at issue, and data are collected from samples of persons presumed to be drawn from something called the "general population." Stratified sampling may be resorted to, to represent this general universe of persons as faithfully as possible. We believe that, instead of seeking to reproduce the fiction of a "general population" by representative sampling procedures, more useful data could be reached, even at this point, by employing *structured* samples of persons, of the kind noted in chapter iii. It seems clear that when "effects" can be specified, such as those of socio-economic class, age, educational status, and the like, these should be represented formally in "samples" of persons and not left to the vagaries "of chance," or in such a form that interaction between the effects is of unknown amount. But we have no wish to press this matter at this juncture.

More often, questionnaires are examined in relation to external criteria of some kind, represented in groups of persons. Thus in the case of *G-3*, a sufficiently large number of *overachiever* students may be compared with a large enough number of *underachievers*, to determine which questions of the questionnaire discriminate between the two groups. The method is that of *gross mean differences*, to

which the *discriminative function* or other statistical devices can be applied for some proof of the differences. Validating studies of this kind are resorted to very commonly. Thus we have available some data for 300 students, 150 overachievers and 150 underachievers,[2] from which it is not difficult to determine which of the *G-3* questions are discriminative. These particular questions would be earmarked for future use, as prognostic or the like for *over-* or *underachievement*. All other questions, not significant, would be discarded as far as this criterion of achievement is involved. They might, of course, be discriminative in some other respects, for other groups of persons.

The logic is thus centered upon the separate questions, whose discriminability is studied with respect to *classes* of persons. We might end, for example, with some information of the kind that questions a, b, c, . . . , distinguish between the sexes; questions g, h, i, . . . , between students from Georgia compared with those from New York State; questions l, m, n, . . . , between city and rural students; questions p, q, r, . . . , between football players and chess devotees; questions x, y, z, . . . , between students who "date" a lot and those who do not; and so on. The elegancies of the *discriminative function* can help out: but the end-product is still merely a catalogue of interesting, but disparate and unrelated, facts.

Q-TECHNIQUE APPROACH

How, then, are we to study a questionnaire, such as *G-3*, along Q-technique lines? We would first seek to define the universe or universes of statements involved, each of which should satisfy the following conditions:

1. The statements in each universe should be of one class.
2. The *transitory postulate* should apply sensibly to any sample of statements.
3. The statements should be permitted to interact freely.
4. The *means* for different persons should contain no important information.

Little can be gained in systematic work if any odds and ends of questions are to be regarded as a sample for Q-technique purposes, just because there is a large number of them. We look instead for a

2. The categories were based upon the students' scores in the College Examination Board's *Scholastic Aptitude Test* and upon their success or failure at the conclusion of their first year of college work.

certain homogeneity, such that allows us to say that all the questions or statements of a sample are alike, having some common point of resemblance which is likely to be of pertinence for scientific purposes. All the questions in the *General Questionnaire G-3* have to do with students and college matters, but a glance through the various questions suggests at once that two different classes are involved, one concerning student *motivation* toward college life and the other dealing with more particular *study habits*, as we have previously suggested.

It is reasonable to suppose that the *transitory postulate* may apply within either such class, but not for their gross admixture. It is a little difficult, to say the least, to compare the great values of life with whether one takes down notes or not at lectures, and, although the latter may depend upon the former, this is not at issue at this juncture.

Similarly with respect to functional or actual *interaction* between the questions: the aim is to permit questions to have quite different "meanings," if need be, in different situations. The same statements, for example, can "take on" very different significances under different conditions of instruction, for one and the same sample of statements. Or one statement, appraised in isolation, may be given a significance it rarely has when it is judged along with the others. Thus if we ask college women whether they would continue to study if married, most say "Yes." But when the statement is placed alongside many others, as in Q-studies, it loses importance for conscientious students but is placed *high* by women who are failing courses—*high*, because of a deep wish they have, apparently, to be married and away. This postulate of interaction is important because it makes no extramural suppositions about what a statement might mean "in general," or "on the average," or according to a presumption of the "rule of the single variable," or according to assumptions about *individual differences*.

Reference has been made already to the way in which information may be lost in correlational analysis unless steps are taken to deal with the question of differences in the *means* for each person's arrays in Q-technique. The matter is important, methodologically, and we are tempted to give it form by calling a *mean* of zero, when it has been reached without throwing away important information, a

"distensive zero." All the information, so to speak, bulges out or distends from it—it is all contained in the dispersion about zero, that is, in the variance. The matter was referred to as postulate vii of the quantitative principles both for R and for Q in chapter iii (pp. 58 f.).[3]

It was possible for *G-3* to put together two sets of statements with a reasonable expectancy that the above postulates would be at issue. One set concerned *study habits*, the statements being of the following kind:

I can study almost anywhere, with "noises off," in a noisy room, etc.
I can't write fast enough.

The other dealt with the broader motives, with statements of the following kind:

I like to talk about boy friends, dances, popular phonograph records, and the like.
I have a strong interest in a career.
I don't want to marry, and am planning a business, professional, or social career on that basis.

A sample of 70 was taken for the former and 160 for the latter. The *distensive zero* is taken care of, usually, when a balanced structured design is employed. In the case of the sample for *study habits*, a simple design was suggested, for one effect at two "levels," namely: (i) statements which refer to efficient study and (ii) others indicative of ineffective study. The sample of 70 contains 35 questions at each level, so balancing it about efficiency-inefficiency. As a basis of assessment, any student could be asked to read through all 70 statements (previously thoroughly shuffled), and to offer descriptions for questions of the following kind:

i) What is your *characteristic* mode of study?
ii) What are the *ideal* student's study habits like, i.e., the most effective student's?
iii) What is most likely to be *ineffective* in study?

A suitable frequency distribution for this sample is given in Table 1.

3. In R-methodology, it will be remembered, the *mean* scores gained on mental tests are arbitrary, depending on the length of the tests. The correlation coefficient is therefore a statistic which will deal with such scores in all their important respects when it reduces these, as it does, to *standard* ones of mean *zero* and standard deviation 1.0. Steps have to be taken to reach such conditions in Q-technique.

Many more questions were available concerning motivation, and it seemed of theoretical interest to structure these in relation to M. D. Vernon's earlier studies (194) on the motives of women students at college. Vernon gave reasons for supposing that eight classes of motive are likely to be observed, namely:

a) Humanitarianism
b) A need for independence
c) Delight in activity
d) A will for dominance
e) Social conformity
f) Display
g) Pleasure and variety
h) A need for security

Each can be represented at two "levels," one having a *positive* import and the other a *negative* one: that is, with respect to *a* a positive statement would be of the kind "I am interested in the welfare of the community," whereas a negative statement would be of the kind, "Let everyone mind his own business, I say." Ten questions were

TABLE 1

Score	10	9	8	7	6	5	4	3	2	1	0	
Frequency	2	3	5	8	11	12	11	8	5	3	2	($n=70$)

taken at each "level," for each of the eight "effects" *a–h*, making 160 statements in all. The "sample" is therefore simply structured, not a balanced factorial design.

We are now ready for Q-technique studies. Two samples now replace the *General Questionnaire G-3* but are composed of its questions. It is our experience that almost any questionnaire can be replaced, for experimental purposes, by a sample or samples of the above kind, whether structured or not. These indeed merely represent some of the effects implied in the questionnaire itself—for why indeed should study habits be at issue, other than in terms of implications about effective or inept habits? And if prior work has suggested some main classes of motivation, is it not wise to see whether these apply to the present case? We have yet to see a questionnaire which cannot be made more explicit and formal, for Q-purposes, along some such lines as the above.

FACTORS IN STUDY HABITS

Sight has not been lost of the general purpose of this chapter, which was to consider how to discover what is inherent in a question-

naire, along Q-technique lines. We shall proceed, therefore, to a detailed examination of some data for the samples just considered, beginning with that about *study habits*. Data will be considered for 17 men and 12 women students who gave descriptions of their characteristic study habits in terms of the sample of 70 statements referred to above. *The proposition is that the Q-analysis of these 29 persons will provide the essential psychological facts that are likely to be at issue about study habits, as far as these questions are concerned, and that larger numbers of persons are not essential to such facts.* The concern will be with a few *overachiever* and *underachiever* students. But, whereas these were criteria classes for the method of gross averaging (p. 193), against which the questions in the questionnaire could be evaluated, Q-analysis will tell us, instead, what classes or types of persons really exist, in terms of the questions themselves.

In Q-technique we are free to choose the experimental persons with an eye to validation possibilities. They need rarely be chosen at "random" from the "general population," whatever that may be. Thus the 17 men whose data we are to examine consisted of three groups, as follows:

a) 5 were students at Princeton, known to be *overachievers* according to objective criteria (Nos. 1–5).
b) 7 were students, again at Princeton, who were *underachievers* according to the same criteria (Nos. 6–12).
c) 5 were research members of the Educational Testing Service at Princeton (Nos. 22–25).

These are in no sense random selections from any "general population" but were chosen, instead, to test a proposition, namely:

Proposition 1: Groups *a* and *c* will be *alike* with respect to study habits (present or in retrospect), and *b* will be *unlike* them.

The statistician would take the view that any facts discovered for these few persons would permit valid inferences to be drawn about them, but not about other persons, unless some sampling conditions have been specified. Inferences are drawn, however, on nonstatistical as well as statistical grounds, and we shall be so bold, later, as to appeal to such wider grounds for our conclusion that the factors we find for these few persons will occur for any other students in the country. But, clearly, each category, *a*, *b*, or *c* above, implies a more general body of persons of the same class—in case *c* all success-

ful students, past or present, are implied; and in the others, *a* and *b*, two classes of students which can be operationally defined. But we have given no heed to the selection of the particular persons for our study. The very first five Ph.D. students we could lay hands on served for *c*, and first-year students at Princeton for *a* and *b*. If general conclusions are drawn from their results, they must be on grounds which depend as much on the total *scientific situation* (Kaufmann, 92) as upon any formal sampling conditions as far as persons are concerned. The grounds, indeed, will be in terms of the factors, their *simple structure*, if any, and the insights they provide. All these, in our view, comprise the scientific situation along with the credibility of these classes of persons as such; an abduction will be drawn from this total situation and not from statistical inference alone.

With respect to women: 6 *overachiever* and 6 *underachiever* women were taken from a college list at New Brunswick, New Jersey, the criteria for over- and underachievers being the same as for the men. The 5 men of *c* were added to the table for these 12 women, so as to provide a concrete illustration of the relative *invariance* of the men's factors when they are put in a different matrix.

THE MEN'S DATA

Each of the 17 men made two appraisals with the sample of 70 statements, namely: (1) an account of his own characteristic study habits and (2) a description of the habits of an "ideal" student. In addition to the proposition that successful students will differ from unsuccessful ones with respect to these study habits and attitudes, we can now add another, namely:

> Proposition 2: Greater discrepancies will occur between "self" and "ideal" appraisals for *underachievers* than for *overachievers*.[4]

Table 2 gives correlation coefficients for instruction 1, i.e., for characteristic study habits. Along lines of Spearman analysis, two orthogonal factors are indicated, I and II, and their loadings for the 17 men are shown. The Thurstone centroid method, with subsequent

4. We shall not deal, in detail, with data for the "ideal" appraisals or with empirical testing of this proposition No. 2. The data, in fact, support this proposition, but for reasons of space they must be omitted here. It is sufficient to provide a hint of these possibilities for methodological purposes.

TABLE 2

CORRELATIONS FOR 17 MEN, n = 70 STATEMENTS ON STUDY HABITS; SELF-CHARACTERISTICS

	Research Workers					Overachievers (OA)					Underachievers (UA)							Spearman Factors		Thurstone Factors			Simple Structure		
	22	23	24	25	26	1	2	3	4	5	6	7	8	9	10	11	12	I	II	I′	II′		I	II	
22	—	52	60	21	40	37	44	42	34	04	26	07	22	00	−12	−14	−20	66	24	71	−08	22	X	0	Research workers
23		—	35	11	34	36	37	53	19	22	29	22	19	07	11	−03	−01	53	00	60	06	23	X	0	
24			—	22	66	51	52	52	63	21	42	25	38	05	11	08	01	82	00	82	13	24	X	0	
25				—	38	01	28	24	32	00	15	15	28	35	15	20	33	33	27	18	41	25			
26					—	41	58	55	50	12	36	28	19	27	11	25	34	78	09	66	33	26	X	0	
1						—	46	42	24	32	35	09	28	08	00	−03	−05	53	00	66	00	1	X	0	Overachievers
2							—	49	48	22	45	23	34	24	16	19	17	73	15	67	27	2	X	0	
3								—	39	34	41	46	26	24	31	20	24	71	24	67	34	3	X	0	
4									—	03	51	34	30	32	17	23	24	60	29	52	38	4			
5										—	18	19	04	03	24	22	17	08	24	27	18	5			
6												37	38	28	36	28	10	56	33	49	37	6			
7													17	25	50	41	48	36	51	22	56	7			
8														22	26	31	14	43	27	31	37	8			
9															38	32	53	09	56	05	61	9	0	X	Underachievers
10															—	43	47	05	75	02	66	10	0	X	
11																—	61	+05	73	−03	70	11	0	X	
12																	—	+05	70	−09	77	12	0	X	

rotation to maximum zero loadings, provides substantially the same two factors I′ and II′, as well as a third (III′), which is of dubious significance and which we shall ignore. It would in no way alter the argument, even if it were significant. The Thurstone factors I′ and II′ reproduce the correlations rather better in detail than do the Spearman factors I and II; but both are really dealing with precisely the same matters. Factors I and I′ are obviously centered upon the *overachievers*, whereas factors II and II′ are as clearly focused on the *underachievers*.

Seven of the effective students and four of the underachievers provide clear-cut *simple structure*, the two factors being orthogonal. Thurstone's methodology, in R-technique, has been to search for just such structure and to discard tests which break it (191). We are in the same position, except that there is of course no need to discard anyone. The respective study habits represented by these factors are not in a bipolar relationship—overachievers are not just the precise opposites of *underachievers*. Again it is possible only to indicate one's respect for the logic of such structure: even the exceptions to it are explicable and obvious. Research worker No. 25, for example, was whimsical, a little resistive or negative, and gave an account of study habits befitting such a mood—he projected upon the statements something of his own whimsicalness.

Factor I indicates what a successful student is likely to say about his characteristic mode of study: its factor-array begins as follows:

MOST CHARACTERISTIC STATEMENTS

No.	Score	
63	10	"I feel somehow excited and interested after a period of study."
12	10	"I usually read or study rather more than an assignment demands—e.g., I become interested in particular points in an assignment, and often pursue these further."
10	9	"I usually find something valuable in every course I take."

LEAST CHARACTERISTIC STATEMENTS

13	0	"I feel that I may have chosen the wrong courses."
50	0	"I think I lack some confidence in my ability to do satisfactory academic work."
69	0	"I am unfairly handicapped because I can't do well in written, essay-type exams."

Factor II's *factor-array* proceeds as follows:

MOST CHARACTERISTIC STATEMENTS

No.	Score	
7	10	"Outside interests certainly interfere with my studying—I never do as much, on this account, as I should have done."
61	10	"I study more efficiently just before an examination."
1	9	"I'm afraid I never work hard enough—that's my trouble."

LEAST CHARACTERISTIC STATEMENTS

No.	Score	
62	1	"I take rough notes of a class lecture, and then write them out more definitely later on, usually that evening."
4	0	"I have a wretchedly poor memory."
16	0	"I would rather spend vacation quietly studying than having a part-time job."

We assert a solidity, concreteness, and reality for such factors. Nothing arbitrary is involved, such as Burt (41) believes factors to be. These factors really do represent beliefs about the *study habits* of effective and ineffective students, respectively. No student dominantly of Factor II is ever likely to be an *overachiever;* and no overachiever is ever likely to be of factor II type. But students of type I may be underachievers.

DATA FOR WOMEN

Table 3 lists the correlation coefficients for the 12 women and the 5 men research workers, again for their self-characteristics in study habits. Spearman and Thurstone forms of analysis point to the same factors, and again two provide an adequate solution; there is a third factor for the centroid analysis, of minor, scarcely significant, dimensions, which we shall ignore. Again there is *simple structure*, factor I_a or I'_a centered on the *overachiever* women and the research workers, and factor II_a and II'_a on two of the underachiever women. The men have factor I'_a in the same relative amounts as they had factor I in Table 2. The respective factors for men and women are very similar, except for a slight sex difference: the women appear to be rather more conscientious or conscience-stricken, or both.

Factor I_a for the women, for example, begins as follows:

MOST CHARACTERISTIC STATEMENTS

No.	Score	
47	10	"I am usually fairly-well prepared for the classes I attend."
10	10	"I usually find something valuable in every course I take."
3	9	"Naturally I exert strong effort to do good work in my courses."

TABLE 3

CORRELATIONS FOR 5 MEN AND 12 WOMEN, n = 70 STATEMENTS ON STUDY HABITS

	Research Workers (Men)					Overachiever Women						Underachiever Women						Spearman Factors		Thurstone Factors			Simple Structure	
	22	23	24	25	26	1	2	3	4	5	6	7	8	9	10	11	12	I_a	II_a	I'_a	II'_a			
22	—	52	60	21	40	25	32	12	23	33	01	27	29	−03	−06	−02	23	54	15	62	02	Research workers	X	0
23		—	35	11	34	14	24	26	34	30	12	28	29	17	−03	01	17	47	00	52	11		X	0
24			—	22	66	42	48	34	54	62	12	34	58	15	10	05	28	84	00	80	20		X	0
25				—	38	06	31	32	33	20	−09	04	21	06	04	−02	22	38	06	37	08		(X)	0
26					—	16	28	24	71	55	02	24	46	27	−06	−07	31	71	11	83	00		X	0
1						—	39	33	13	26	23	34	56	−08	15	17	20	44	08	29	41	OA		
2							—	49	33	46	26	19	44	33	17	30	50	63	27	41	57			
3								—	34	42	34	29	22	49	29	14	30	51	27	27	62		0	X
4									—	49	03	30	48	45	15	10	24	66	15	71	20		X	0
5										—	18	22	40	37	06	14	26	69	00	62	31		X	0
6											—	14	17	20	13	12	04	06	30	02	40		0	X
7													34	14	35	30	34	67	03	57	34	UA	X	(0)
8														14	22	11	32	43	39	27	47		(0)	(X)
9														—	18	31	20	46	28	28	48		(0)	(X)
10															—	23	11	34	01	21	44		(0)	(X)
11																—	37	05	58	−02	46		0	X
12																	—	04	77	−08	56		0	X

LEAST CHARACTERISTIC STATEMENTS

69	1	"I am unfairly handicapped because I can't do well in written, essay-type exams."
13	0	"I feel that I may have chosen the wrong courses."
4	0	"I have a wretchedly poor memory."

Factor II_a proceeds as follows:

MOST CHARACTERISTIC STATEMENTS

No.	Score	
36	10	"I would continue my studies even if I were married."
61	10	"I study more efficiently just before an examination."
1	9	"I'm afraid I never work hard enough—that's my trouble."

LEAST CHARACTERISTIC STATEMENTS

42	10	"Dull and ineffective teaching is the main reason for my poor scholastic record."
56	0	"My instructors don't seem to care whether I learn or not."
53	0	"People respect you more if they believe you don't study a great deal."

DISCUSSION OF RESULTS

The same factors appear for the men and the women, and it is apparent that diligent or effective students could scarcely have habits or attitudes about study other than those arrayed in the order that comprises factor I. The same credibility attaches to factor II for the ineffective student. The factors are realistic, concrete matters. Moreover, the factors are achieved in *simple structure* and are validated very considerably by the objective measures of *over-* and *underachievement*. We infer, on such grounds, that these same factors, I and II, or closely related ones, will be found extensively among students, wherever they are tested under conditions of the kind here contemplated, up and down the country, in New Mexico or in New York State. No doubt minor factors can be brought to light in addition to these two, and different groups of students might slant the factors one way or another. For immediate purposes, however, two clear *types* of students can be operationally defined in terms of these study-habit statements—the efficient type I and the inadequate type II. No person of type II is ever likely to be an *overachiever*, but students of type I may be *underachievers*. These types, in the *simple-structured* case, must be regarded as "pure." But some students may be of neither type, and others of "mixed" variety, having both factors I and II. The data for all the variates may thus

be a case of *simplest structure*, as we defined it in chapter vi. We do not say so at this point because it would have to be shown that explanations for I and II really help us to understand the cases involving *both* factors, as well as those having neither. This is a matter for separate consideration. It is also a separate issue to determine how many students are likely to be of types I, II, neither, or mixed. The factors I and II, however, are not "extreme cases." They represent *communality*, and, if only two pure cases issue from every dozen or so of *overachievers* or efficient students and two from *underachievers*, large numbers of students are likely to be involved in the long run. An "extreme case," on the contrary, would be one like No. 25, who has a Ph.D., is a brilliant researcher, and yet has the oddest notions about how to study. He is contrary and, in relation to the scientific situation, an "extreme case."

From our standpoint, the *General Questionnaire G-3* had inherent in it, as a basis of classification, these typological matters; and these, we believe, could have been taken into account in the construction of the questionnaire in the first place, which could have been used for indicating a person's type (I, II, neither, or mixed in the above case). How to indicate such types on a large scale, however, has never been attended to seriously up to now by anyone in psychology. Clearly, detailed Q-sortings are impossible on a large scale, unless they are dressed up as a sort of Canasta game. But there can be little doubt that practical methods can be devised, within the framework of existing questionnaires, to indicate such types, at least approximately. Thus, instead of inviting the individual to grade each question of a questionnaire separately from 1 to 5, as in the *General Questionnaire G-3*, it would be simple to confront him with, say, sets of six questions at a time, as in the following example:

INSTRUCTIONS

Rank the following statements in order from the one (1) MOST characteristic of you, to that (6) LEAST characteristic of you:

a) "I like to use odd moments, like time between classes, to review what I have learned."
b) "I study more efficiently just before an exam."
c) "I sometimes feel discouraged after a session of studying."
d) "I find it difficult to make up missed assignments."
e) "I feel somehow excited and interested after a period of study."
f) "I worry a bit about my course work."

The six statements are a Q-sort in miniature; but it is arranged from prior analysis that two of the statements are highly discriminative for factor I (*a*, *e*), and two for factor II (*b*, *d*), the other two being nondiscriminating. Any student ranking *a* and *e* high would gain a mark or score indicative of type I; if *b* and *d* are ranked high, type II is indicated. Given, say, ten such sets of comparisons, it is reasonable to suppose that the main types will be fairly well accounted for. In short, the old-type questionnaire could be restructured, to bring these types to light.

THE "IDEAL" STUDENT

Each subject gave a description of what an "ideal" or really efficient student would be like, in terms of the sample of study-habit statements. Table 4 gives the correlations for these "ideal" descriptions, for the 17 men under consideration. Most of the variance is taken up by one factor (D_1), and there is nothing that one can do to separate the *overachievers* or efficient students from the *underachievers* by rotating the two factors D_1 and D_2. Factor D_2 is scarcely significant; but, such as it is, efficient and ineffective students participate in it alike. The Ph.D. No. 25 again separates himself from the others of his particular kind.

Table 5 shows the correlation coefficients for women students, and again one factor alone takes up most of the variance. The factor E_1 is rotated through the five Ph.D. students, as it was for factor D_1 of the previous table; it will be noticed that most of the loadings for D_2 (for the men) in Table 4 are negative, whereas, here, for E_2 they are all positive. The difference is attributable in part to the sex differentiation. Again, however, the overachievers and the underachievers are not distinguisable in terms of these factors.

The factors D_1 and E_1 differ very little from each other, and they are not very different, either, from the factors I and I_a for self-characteristics. Yet, whereas the latter discriminate between overachievers and underachievers, the former do not. Clearly, *underachievers* have much the same idea of an efficient student as have *overachievers*. In the case of the men, the underachievers have, perhaps, rather smaller factor loadings, indicating that their ideas about effective study habits are not so precise as they might be.

It is of interest to look at the correlation coefficients between *self-*

TABLE 4

Correlation Coefficients for 17 Men, n = 70 Statements on Study Habits Assessed for "Ideal" Student

		22	23	24	25	26	1	2	3	4	5	6	7	8	9	10	11	12	Thurstone Analysis (Centroid) Factors Rotated D_1	D_2	
Research workers	22	—	67	70	63	68	57	68	66	58	07	50	58	56	58	57	37	64	81	−01	Research workers
	23		—	66	57	68	68	60	59	52	15	59	54	52	62	51	45	65	79	−01	
	24			—	61	65	68	69	71	57	19	50	62	51	54	44	45	68	82	−03	
	25				—	56	62	61	62	50	15	51	50	68	52	54	37	53	69	−25	
	26					—	70	69	70	58	27	53	65	57	62	51	42	65	82	−07	
OA	1						—	81	65	62	24	64	58	66	61	71	50	69	81	−32	OA
	2							—	74	67	21	63	57	65	61	73	44	69	82	−33	
	3								—	65	23	63	67	64	52	63	41	55	79	−27	
	4									—	09	47	64	70	59	51	34	62	75	−11	
	5										—	29	14	36	09	32	−04	13	15	−44	
UA	6												55	52	40	64	37	56	64	−39	UA
	7													55	58	57	32	53	75	−08	
	8														59	62	28	59	70	−40	
	9															60	42	68	78	04	
	10															—	32	58	66	−50	
	11																—	57	56	08	
	12																	—	84	04	

TABLE 5

CORRELATION COEFFICIENTS FOR 12 WOMEN AND 5 MEN, $n = 70$ STATEMENTS ON STUDY HABITS, ASSESSED FOR "IDEAL" STUDENT

		22	23	24	25	26	1	2	3	4	5	6	7	8	9	10	11	12	Thurstone Analysis (Centroid) E_1	E_2	
Research workers	22	—	67	70	63	68	58	56	71	62	65	64	65	53	56	64	68	57	82	01	Research workers
	23		—	66	57	68	49	54	70	58	65	55	63	55	56	65	68	57	82	−04	
	24			—	61	65	67	64	71	69	65	54	67	61	66	61	74	62	82	12	
	25				—	56	60	61	59	64	58	62	61	60	60	67	54	53	68	36	
	26					—	52	52	63	66	76	61	62	58	66	67	73	54	83	02	
OA	1							63	64	71	66	60	66	60	72	67	61	74	68	51	OA
	2							—	70	58	62	54	69	62	62	66	56	68	70	36	
	3								—	64	75	60	80	66	68	64	73	69	87	08	
	4									—	75	61	70	65	67	69	63	65	77	32	
	5										—	63	75	61	66	64	70	63	84	10	
	6											—	57	59	59	69	60	44	69	29	
UA	7													62	71	64	68	65	83	17	UA
	8														49	64	59	57	69	32	
	9															73	65	65	74	33	
	10															—	63	65	73	45	
	11																—	70	85	02	
	12																	—	72	32	

characteristics (S) and the "*ideal*" (D) accounts, for each student in turn. The coefficients (r_{SD}) are listed in Table 6.

It is apparent that the efficient students tend to have high correlation between their *self* and their *ideal* representations; the reverse is the case for the ineffective students (UA). The students we have just seen are not distinguishable in terms of the factors D or E; we can conclude, therefore, that the *underachievers* certainly know what

TABLE 6*

Men				Women			
Student No.	r_{SD}	Student No.	r_{SD}	Student No.	r_{SD}	Student No.	r_{SD}
22	85	6	60	1	62	7	49
23	64	7	26	2	72	8	50
24	86	8	55	3	34	9	26
25	49	9	09 UA†	4	55 OA	10	17 UA
26	79	10	−06	5	57	11	−01
1	58	11	−10	6	05	12	13
2	74	12	−12				
3	60 OA						
4	58						
5	36						

* Decimal points are omitted.
† OA = overachiever; UA = underachiever.

sound study habits are; but they appear not to live up to this insight. Proposition 2 (p. 199) is therefore confirmed.

THE SAMPLE ON MOTIVATION

The sample of 160 statements, composed of questions taken from the *General Questionnaire G-3* concerning reasons of a general nature about a student's motivation for college, was simply structured, it will be recalled, with respect to the following areas of motivation:

a) Humanitarian
b) Independence
c) Activity
d) Dominance
e) Social conformity
f) Display
g) Pleasure and variety
h) Security

Each area was represented at two "levels," for *positive* and *negative* assertions, respectively, so that the design is as shown in Table 7. There were 10 statements at each "level" for each "effect." If "lev-

els" are designated A, and motives B, the following analysis would normally suggest itself (Table 8). It would be arbitrary, however, to make such an analysis because the variances *within* B are unlikely to be homogeneous: the effects looked for, indeed, consist of differential separations in the "levels" for the various "effects." The

TABLE 7

Motives (B)	"Levels" (A)		
	Positive	Negative	
a)	10	10	
b)	10	10	
c)	10	10	
d)	10	10	($n = 160$ statements)
e)	10	10	
f)	10	10	
g)	10	10	
h)	10	10	

TABLE 8

	D.F.
ΣA	1
ΣB	7
$\Sigma(A \times B)$	7
Σ Replication (16×9)	144
Total	159

TABLE 9

Motives (M)	Positive Level (P)
a)	10
b)	10
c)	10
d)	10
e)	10
f)	10
g)	10
h)	10

analysis in Table 9, therefore, is more to the point. This table is concerned with the *positive* level; there would, of course, be an analogous one for the *negative* level. There are 80 statements, i.e., 79 degrees of freedom, for each table.

The *sums of squares* $(\Sigma d_1)^2$ for the 80 statements of the *positive* level will then be divisible into two parts, as follows:

$$\left.\begin{array}{lcccc} & \Sigma(d_1)^2 = & \Sigma(\text{between } M) & + & \Sigma(\text{within } M) \\ \text{Degrees of freedom} & 79 = & 7 & + & (9\times 8) = 72 \end{array}\right\}. \quad \text{(i)}$$

The *sums of squares* $\Sigma(d_2)^2$ for the 80 statements of the *negative level* will provide a similar division, namely,

$$\left.\begin{array}{lcccc} & \Sigma(d_2)^2 = & \Sigma(\text{between } M) & + & \Sigma(\text{within } M) \\ \text{Degrees of freedom} & 79 = & 7 & + & 72 \end{array}\right\}. \quad \text{(ii)}$$

It is possible to judge whether ΣM is significant (i.e., for the two *levels*), in terms of the expectancy provided by 144 degrees of freedom, i.e., the 72 from each of the above separate tables.

This variance design is referred to particularly as a reminder that the specification of the *error* expectancy is of first importance in Fisherian design. Clearly, all ten humanitarian statements (*a*) (*positive*) could gain high scores in a Q-sorting, and all ten *negative* ones for this motive, low ones. This is the kind of significant effect we look for and expect. The replication is therefore with respect to the "levels," *positive* and *negative*, respectively, for each motive. It is worth giving a reminder, in this connection, that more elaborate devices for achieving efficient estimates of error can be employed in our situations, every bit as much as in agricultural experimentation —*confounding*, the *Latin square*, and like designs are all available and are applicable.

TABLE 10

Score	0	1	2	3	4	5	6	7	8	
Frequency	4	6	9	13	16	13	9	6	4	(n=80)

We are ready, then, for experiments. The 160 statements were divided into two sets for ease of application, each containing 5 *positive* and 5 *negative* statements for each of the eight motives. The 24 *overachiever* and *underachiever* students referred to above gave descriptions of their self-characteristics in terms of these statements, using the frequency distribution shown in Table 10. Having sorted the two sets and with both sets laid out before him, thus sorted into

nine piles each, each student next resorted all 160 into one distribution, on the basis of the slightly broader distribution (Table 11).

We can now analyze the data for each student separately. We first prove for each student whether his Σ(between M) is significant, for an error expectancy with 72 degrees of freedom. If it is, then likewise the differences between the *means* for the eight motives can be determined, to see which provide the large differences. Thus, in

TABLE 11

Score	0	1	2	3	4	5	6	7	8	9	10	
Frequency	6	8	12	18	22	28	22	18	12	8	6	($n=160$)

the case of one student, we found that Σ(between M) was significant, and that the *means* for motives a and c were significantly greater than those for d and f, i.e.,

$$a = c > d = f \,,$$

$$a = c \text{ was } \textit{not} > b,\ e,\ g,\ h \,,$$

$$d = f \text{ was } \textit{not} > b,\ e,\ g,\ h \,.$$

It is clear, then, that whatever is significant can be determined for each student in turn, with respect to data for both "levels." Finally, those students who show similar results may be classified into types.

Obviously, a lot of detailed statistics is involved, but of a routine nature, and it is not practicable to present it all here. It is sufficient to indicate the results obtained without going into detail, since methodological matters alone are at issue.

It seemed reasonable to distinguish *four* classes or types of men and *five* classes of women students in terms of the above variance analysis. We shall content ourselves, for the moment, by indicating which students[5] fell, respectively, into which class (Table 12). The outcome of the variance analysis, then, is a classification of the students with respect to known, i.e., proved, effects.

We next turn to the correlations. Tables 13 and 14 give the correlation coefficients and results of a factor analysis for the men and women students, respectively. Two factors serve sufficiently, in

5. Students 1–12 are the same, student for student, in all the tables in this chapter, men and women.

TABLE 12

	Women		Men
i.	Nos. 2, 8	i.	Nos. 1, 2, 5
ii.	Nos. (1), 4, 6, 7	ii.	Nos. 3, (11)
iii.	Nos. 3, 5, 10	iii.	Nos. 4, 7, 8, 10, 12
iv.	Nos. 9, 11	iv.	No. 9
v.	No. 12		

TABLE 13

CORRELATION COEFFICIENTS FOR 12 STUDENTS, MEN
(n = 160 Statements on Motivation)

	1	2	3	4	5	6	7	8	9	10	11	12	Thurstone Factors (Centroid) (Rotated) α_1	α_2	
1	—	46	44	27	32	10	15	25	−04	11	14	02	62	−01	OA
2		—	55	45	40	30	31	41	09	18	30	21	73	23	
3			—	50	30	26	53	55	16	25	24	33	60	45	
4				—	19	25	54	45	20	25	18	34	37	57	
5					—	01	12	21	−13	19	29	26	58	00	
6							26	22	10	28	18	15	18	36	UA
7							—	46	12	27	22	43	27	62	
8								—	07	33	17	38	38	53	
9									—	12	04	14	−05	30	
10										—	26	54	17	58	
11											—	40	39	24	
12												—	21	62	

TABLE 14

CORRELATION COEFFICIENTS FOR 12 STUDENTS, WOMEN
(n = 160 Statements on Motivation)

	1	2	3	4	5	6	7	8	9	10	11	12	Thurstone Factors (Centroid) (Rotated) β_1	β_2	
1	—	13	17	17	28	28	22	28	17	−04	23	05	16	36	OA
2		—	35	44	39	40	37	57	27	36	44	11	64	32	
3			—	17	37	18	20	44	22	36	35	01	62	05	
4				—	19	30	49	44	25	22	34	19	32	53	
5					—	28	17	36	32	25	26	01	58	11	
6						—	34	40	19	05	42	14	25	54	
7							—	33	30	23	36	22	26	62	UA
8								—	35	22	51	02	62	37	
9									—	16	23	16	40	27	
10										—	13	11	51	00	
11											—	05	42	45	
12												—	04	27	

each table, to subsume the correlations. Rotated, the loadings for the men are given at α_1 and α_2, and for the women at β_1 and β_2. The situation is now not so clear-cut as it was for the study habits, but there is some indication that factor α_1 is associated with overachievement, and α_2 with underachievement, more especially for the men (Table 13). The matter is not so clear, however, in the case of the women students.

There has been evidence, already, that the men comprise a more obvious set with respect to *under-* and *overachievement:* the *underachievers* at Princeton were frankly there for purposes other than academic. But in the case of the women, only one, No. 12, was of this class, and her results support this throughout. The truth is, however, that attention to the *factors* as such, without regard to the loadings, is not a satisfactory basis for comparing the students.

TABLE 15

Type	Student No.	Factor Loadings	
		α_1	α_2
i	1	62	−01
	2	73	23
	5	58	00
ii	3	60	45
	(11)	39	24
iii	4	37	57
	7	27	62
	8	38	53
	10	17	58
	12	21	62
iv	9	−05	30

TABLE 16

Type	Student No.	Factor Loadings	
		β_1	β_2
i	2	64	32
	8	62	37
ii	1	16	36
	4	32	53
	6	25	54
	7	26	62
iii	3	62	05
	5	58	11
	10	51	00
iv	9	40	27
	11	42	45
v	12	04	27

We remember that the prior and independent variance analysis for each student in turn has resulted in the classification of the men and women into four or five types. Let us compare, then, these classes and the factor analysis. The data for the men are given in Table 15. Those for the women students are given in Table 16. That is, although there are only two factors, more than two types of students can be indicated in terms of them. *The two factors "hit" accurately upon the various classes, when due account is taken of the*

size of the loadings. In the case of the men we might have been tempted to place No. 9 in class iii, and might have done so, had we not known beforehand from the variance analysis that his data were distinctly different from the others of class iii. Similarly in the case of the women, it would have been impossible to distinguish between Nos. 1 and 12 in terms of the factor analysis; but when we know beforehand that the variance analysis places No. 1 either in a class of her own or approximately with Nos. 4, 6, and 7, and not at all with No. 12, it is adequate so to place her. The wide difference between Nos. 1 and 12 could have been brought to light, otherwise, only if other persons of types similar to No. 1 or No. 12 were in the table.

With respect to the original problem, concerning the prior analysis of the questions in the *Questionnaire*, we can see that, again, if types such as i and iii or iv in the case of men and iv or v for women can be identified, means can no doubt be found for putting such information back into the scoring of the existing *Questionnaire*, or for devising a more suitable form of questionnaire, as indicated by the types, as was suggested for study habits.

THE "JOSTLING" OF EFFECTS

We began by proposing that studies of the kind that questionnaires are meant to serve can often begin more soundly along Q-technique lines. In general, the questions can be analyzed to find factors inherent in them; if any are discovered, it seems reasonable to orient the structure of the questionnaire, or the method of analyzing its data, or both, toward the factors so brought to light. The questions can be designed, as we have seen, along lines suggested by these prior analyses.

But no reference has been made as yet to the way in which results of this kind can be incorporated into a questionnaire so as to cover data for several universes of statements. The original *G-3*, for example, involved two universes at least, one for *study habits* and the other for *motivation*. These have been studied separately, and one might wonder how this squares with our earlier remarks about the "rule of the single variable." The study of propositions about any specified universe of statements in Q-technique involves the "rule" only in so far as the *instructions* for the assessment of a sample may concern themselves with such conditions; but the statements within the sample are in no way subject to such conditions. Moreover, when

pertinent types are known for two or more universes, we could seek to test their relative significance for an individual in a direct operational manner. This can be approached in many ways, but a simple device of the following kind will indicate what we have in mind:

If there are K universes, for which p_1, p_2, p_3, . . . , p_k *person-types*, respectively, are indicated, it is possible to take a significant statement from each *type*, or to make one from each which is discriminative for the *type*, and to invite the subject to rank the set from most to least significant in some respect.

These statements would be heterogeneous with respect to universes, but ranking could be undertaken without a break in the principle of randomization in the acts of judgment (see p. 112), since no two statements occur from the same universe. In practice it is not difficult to achieve situations of this kind, in which the individual has to make a choice among items from different universes, and it is under such conditions that the effects are permitted to "jostle" one another, so that the "rule of the single variable" is outdone. It would take us too far afield to elaborate on this matter, but it is clearly possible to construct different sections in questionnaires which well achieve two objectives—(i) to indicate *types* with respect to any one universe of statements, and (ii) to indicate complexes of such types, as in the case just considered for the "jostling" of effects. Thus, in addition to questions of the kind suggested on page 205 (representing i), the following would be an example of a question to reach into ii:

INSTRUCTIONS

Rank the following statements in order from the one (1) MOST *apposite to your case, to that (6)* LEAST *apposite to you.*

a) I feel somehow excited and interested after a period of study
b) I have always wanted to go to college.
c) I worry a bit about my course work.
d) I study more effectively first before an examination.
e) I like a "crowd"—at parties, bull-sessions, etc.
f) Interested in current affairs.

Items *a*, *d*, and *c* are from the *study-habit* sample, and the others from that for *motivation*. Items *a*, *d*, and *c* are most discriminative for factors I, II, and neither, respectively; *b*, *e*, and *f* types i, ii, and iii of Table 13 (for men). The student is thus confronted, in effect, with a choice among the factors, and we must anticipate that new and significant information is possible along such lines.

PARTITION OF LARGE CORRELATION TABLES

It is rarely necessary, in Q-technique, to calculate all the correlation coefficients possible in the circumstances. An $(N \times N)$ master-table can usually be divided into smaller $(n \times n)$ sections, along lines that the variates themselves suggest, whose analysis can answer most, if not all, the problems likely to be at issue.

Thus, in the foregoing pages, the 29 persons each provided two appraisals[6] in terms of the "sample" on study habits, making 58 variates in all. The master-table would involve 1,653 coefficients. Instead, the table is partitioned along some obvious lines indicated by the variables. Thus we analyzed a (17×17) table for the *men* (136 coefficients), another for women (17×17), but including five of the men (126 new coefficients). This was done for self-characteristics, as well as for *ideals*, although we do not report these data here. In all, only 550 or so of the 1,653 correlations were involved.

No doubt the analysis of the complete (58×58) table would bring some minor additional factors to light, such as would involve sex differences, for example. But the partitioning along obvious lines of cleavage is perhaps sounder in principle. It is sometimes possible to compose smaller tables whose variables are crucial for the effects at issue. Moreover, we may not require a factor I for *men*, and a factor I_a for *women*, divided into $I_x + I_m + I_f$, where x is the communal factor for both sexes and m and f are sex factors. Non-fractioned (170) factors, such as I and I_a, are likely to be more realistic and more pertinent in many studies.

CONCLUSION

We return to the initial thesis, then, that certain kinds of facts which questionnaires may seek to study can be reached along Q-technique lines. It is convenient to distinguish between the operational definition of facts and their enumeration. Thus *age* can be operationally defined, as can *blueness of eyes*. It is not difficult, in principle, to count the proportions of persons in a population having blue eyes or who are of a particular age. Questionnaires, of course, very frequently gather facts of this order, under large-sample conditions in which a population is defined, a sample drawn, and the enumerations made. Our concern has not been with such matters but with the operational definition of other facts. If any important

6. For *self-characteristic* and *ideal* student, respectively.

information of an intrinsic kind is involved in a questionnaire, such as is usually implied in the reasons given for the application of the questionnaire in the first place, then these should be definable, in principle, without using large-sampling methods. It is this possibility that has engaged our attention.

In the example referred to above, the study of only 20 students along Q-lines provided facts of the same order as those reached in the more customary way from the analysis of data for 300 students, divided into two validating groups of 150 students each. Our facts, however, are more satisfying in principle, since there is a rationale for them, and nothing but faith in large numbers and averaging in the other case. It has been argued that factors found in Q-technique for relatively few persons are likely to remain substantially unaltered, under certain conditions, no matter how many persons are considered. Statistical inferences in the Q-technique cases have reference to the intra-individual differences between the statements, and not to the persons. Inferences about the latter and their factors depend upon the total scientific situation. Thus we argue that students are likely to be the same, in America everywhere, and therefore factors I and II for study habits will be elicited anywhere in the country. It may be said, of course, that it is not sufficient to make such an assertion, for who is likely to believe it? The reply to such doubts is readily at hand, however; the Q-sample is available, and anyone can use it to put the assertion to proof for himself under similar circumstances anywhere in the country. So Berkeley, likewise, demonstrated a fact with three bowls of water and his own two hands, that anyone can repeat—if the right hand is placed in a bowl of hot water, and the left in a cold one, after which both hands are plunged into a bowl of lukewarm water, the two hands will have very different experiences of the lukewarm temperature. Similarly for our factors in Q-technique. There is a craving, however, for certainty among men which is sometimes out of harmony with what is sufficient evidence for the time being. Absolute truth, or complete knowledge, is scarcely a practical issue. We are prepared, then, to draw inferences from as few as a dozen persons, under appropriate conditions, namely, such as relate to a general scientific situation. Similarly, factors in R-methodology are frequently accepted, and credited with invariance, for a few *tests* only.

CHAPTER X

APPLICATION TO SOCIAL PSYCHOLOGY

INTRODUCTION

UNTIL very recently, social psychology was the Cinderella of psychology. Now, however, supported by *attitude* testing, *group dynamics*, and *perceptual* theory, it is making forward, and not unmaidenly, advances. There are elements in the gains, however, which scarcely satisfy the sharp scouting of a methodological analysis. We propose to look at these and to outline some of the steps that might be taken, along Q-method lines, to encourage and support an experimental approach to social psychology.

An initial difficulty is of a fundamental kind. In his *Introduction to the Cultural Sciences* (1883), Wilhelm Dilthey (81) distinguished between the natural sciences, which were always "something foreign to us," "external, not internal," and the cultural sciences, which concern "our world of inner experience." This, seventy years later, has a very modern sound. The *subjective* standpoint is now very much in vogue. Hayek (77), to give only one example in social theory, argues eloquently for the subjective character of the social sciences and proposes to set them apart from the physical sciences. Present-day social psychologists in America have likewise adopted a phenomenological approach and concern themselves with the "cognitive structure" (100) of all social interactions. We, too, wish to give every credence to the subjective standpoint—our whole book is so oriented. But for us subjectivity is still behavior. Fundamentally, the social sciences and the humanities and much of psychology are rooted in human behavior, and this includes all man's so-called "inner" life. It is indeed true that religion, art, justice, morality, and the like, for each person, are his own "inner world," fundamentally his "inner experience" in a certain sense. But this is *behavior*, never experience qua experience, as we have seen in chapter v. The same is true of all beliefs, opinions, attitudes, and the like. Thus, although

we agree that it is important to study subjectivity, we suspect that a phenomenological approach to it is misdirected. It is not the case that subjectivity can only be inferred, in principle, because it is forever unobservable, as is assumed by Snygg and Combs (147). Present-day perceptual theories are circumscribed to *a* (i) and *a* (iii) of our representative probes (p. 96); we propose to root them more securely in probes *a* (ii) and *c* (i).

This matter aside, then, we may now turn to the main theme of the chapter, which is to recommend the experimental methodology of Q to social psychologists, as better suited to their needs than R, upon which some reliance has been placed up to now.

ATTITUDES

We make a beginning with *attitudes*, an omnibus term which we shall use to stand for opinions, beliefs, tastes, ideologies, and attitudes in any narrower sense.

It is true to say that large-sample principles have ruled unchallenged up to now in researches and surveys on attitudes. *Scales* are proliferating greatly. They are constructed, at best, by way of the law of comparative judgment, which need not involve person-population principles, although any *use* to which the scales may be put must depend upon suitable *norms*. In every case the supposition is that attitudes are *measurable* for their general implications. According to some authorities, there are difficulties about the "equality of units" in such scales (99), and nowhere is the belief in the necessity of suitably large numbers of cases more widespread than it is among the attitude-testers. In our view all such principles and beliefs are irrelevant for any theoretical or fundamental study of attitudes. It is possible to experiment upon most matters of theoretical interest about attitudes (with all the power of experimental design, variance analysis, and dependency factor analysis to further one's investigations) without the slightest reference to *norms*, nomothetic *scales*, or any *measurements* for individual differences, of the kind that social psychologists have hitherto regarded as essential (23). We need scarcely repeat that the latter investigations are based upon general propositions, and that only *theory* has implications of a general kind, which can be tested under *singular* propositional conditions.

Thus it was asked by Murphy, Murphy, and Newcomb (116): "Will children of parents who show high correlations between two attitudes, themselves reveal higher correlations between the same two attitudes than do unrelated subjects?" These authors correlated 500 sets of parents and their children for two scaled attitudes. Our proposal would be, instead, to start with one family and to confirm the propositions for it. It could be repeated for any other family. But what occurs for one family can have no reference to what happens for another, and the concern is not with using 500 instead of 1 or 2 so that one can "generalize from large numbers." On the contrary, we can determine how far the theory applies to each family separately, and do so in a singular manner, which retains all the possibilities of the theory in all its complexity.

ECOLOGICAL UNIVERSES

One methodological detail requires brief mention before we proceed to elaborate upon the above suggestions. From quite a different angle, Egon Brunswik (35) has aimed at notions which are in many ways basic to some applications in Q-technique. It will be remembered that he arranged for a subject, living and working around the campus at Berkeley, California, to stop at random intervals during the course of a month and note just what was being attended to in the environment at each instant. The sizes of these real things in the world were found to be distributed in a skewed shape, but the logarithms of the sizes were approximately normally distributed. In the same way we once recorded the state of our *mood* at random moments during the course of a fortnight: the result was a highly leptokurtic distribution, with moods of elation and depression at the two extremes and neutral sharply in evidence in the center. Brunswik's example gave rise to the term "ecological universe" to represent the responsiveness of the individual to his environment. The term is just as apposite for moods as for sizes, which, we may hope, are matters of interaction between a person and the things and persons around him—the lovely sunset that points to sublimity, the dirt and despondency of a Chicago slum that depresses, the swishing dress that passes and elates, the dead level of purpose that makes one walk steadily along in a neutral mood—all are instances of the kind that trickle through time in the environmental setting of a person. And

these may be repeated through one's musings and daydreams, which are merely symbolic of interactions.

The same applies to attitudes, opinions, and beliefs. It is conceivable that a recording device could catch every statement made about war or peace, democracy, Semitism, and the like by the members of a family as they sit at meals or are at play or work. It might be only every now and then that most of us would reflect on the horrors of war and the desirability of peace; but others hate war, perhaps, and become militant pacifists, seeing threats everywhere. All such involve the interaction of the person with other persons and things in his environment, whether real or his own fancy, and the behavior may be called "ecological" in the sense that it has a place and encompasses innumerable and diverse behavioral segments for the person in interaction with things and events in his usual habitat.

The concept is important for Q-technique, since it provides a rationale, at times, for putting together a universe of statements about extensive sections of behavior. A sample of these statements will be called "representative" if it in some way samples the behavior adequately, that is, without favoring some parts to the exclusion of others and without omitting whole sections of the universe of possibilities.

THE EXPERIMENTAL STUDY OF ATTITUDES

In what follows we put aside all consideration of the rough-and-ready techniques of pollsters and sociopsychologists. The conception of an ecological universe is well illustrated by our study of the attitudes of our family to "things American." We were newly arrived in this country, and husband, wife, and three children were made the subjects of our inquiries.

The setting was defined to cover the family at work, play, meals, and the like during its first two months in this country. It was natural for us to discuss America, Americans, and their ways; and without the family's knowledge an account was kept of the opinions and attitudes expressed by members of the family. Statements were of this kind: "They dote on children," or "How untidy they are." Statements so gathered represent the naturally occurring situation and may be called "ecological" to distinguish them from samples which we compose on theoretical grounds, such as we suggest later to

test Gorer's (73) theory about the American people. It is possible, of course, to make an analysis of these statements without recourse to additional operations such as we use in Q-technique: a count could be taken, for example, of how many are favorable to America and how many not. The possibility of being more theoretical and penetrating lies in additional operations, such as we employ in Q-methodology. A sample of the ecologically gathered statements can be taken (say, 80 statements), and each member of the family can in turn provide Q-sort over-all descriptions of their attitudes, under different conditions of instruction. The conditions, however, would be those required in relation to some "general theoretic" propositions. These might concern, for example, *identifications* and *prejudices*. Identification would be indicated by factors common, say, to father and daughters or mother and son, and prejudice by the "interpretations" of such factors as occur. The Q-sample could be unstructured to start with (except for attention to the distensive zero, p. 196).

It happened that none of the statements gathered from our family made unfavorable reference to either colored peoples or Jews. It is possible that we were adopting a condescending attitude to minority groups around us (in Chicago), which, in turn, might reflect some feelings of superiority on our part and, in turn, feelings of insecurity. These are theoretical possibilities, based on dynamic theory. They could be stated, however, as propositions:

1. We have a prejudice against Americans (a matter of *opinions* about them);
2. This is a *defensive* action; and
3. Corresponding to it is a condescending attitude toward minorities.

We do not put these forward as models of dynamic principles; they should be regarded merely as the kind of questions that one could raise. The difficulty in the past has been to put such propositions to test, which we have resolved in Q-methodology. Using the selfsame sample of statements, suitable variate designs can be suggested which will serve to put these propositions to test.

In all such investigations, only the particular family would be studied, concretely, in relation to the *dynamic theory*. After all, it would be remarkable if we could measure "defensiveness" for its "general implications," or "condescension" likewise. These principles simply have no such implications.

A STRUCTURED SAMPLE FOR GORER'S THEORY

But this is not to say that we would necessarily set out to study attitudes about America in relation to a particular family only, although the detailed study of a few well-chosen families might be a very excellent way to cover the main dynamics ever likely to be found. There are other approaches, all in Q-methodology, one of which we shall now consider.

Our predilection would be to link up in some way, in any study of "things American," with the accounts given by a De Tocqueville, a Bryce (36), or a Gorer (73). These visitors had much to say, and we shall take Gorer's views in *The American People* (73) as an example.

TABLE 1

Independencies	Levels		No.	D.F.
A, basic mechanisms (unconscious) . . .	(a) Rejection of authority	(b) Dependency (identification)	2	1
B, submechanisms (unconscious) . . .	(c) Desire for love	(d) Fear of rejection	2	1
C, American attitudes .	(e) "Democratic"	(f) "Hardheaded businessman"	3	2
	(g) "Simple guy"			

His theory was that certain attitudes are clearly discernible which are *characteristic* of Americans in a cultural-anthropological setting and which find an explanation in unconscious (mainly psychoanalytical) mechanisms. By "characteristic" he presumably does not mean that all Americans have such attitudes in some degree: but some have, and these are perhaps the more *formative* or the more diagnostic of Americans in some important respects. His standpoint can be represented well enough by the *independencies* for a Fisherian balanced block design in Table 1.

Three attitudes are indicated at C, which are supposedly subserved by the unconscious mechanisms A and B. A balanced block design is at issue, with $2 \times 2 \times 3 (= 12)$ combinations of the independencies one level at a time, namely, the following:

a a a	*a a a*	*b b b*	*b b b*
c c c	*d d d*	*c c c*	*d d d*
e f g	*e f g*	*e f g*	*e f g*

We might decide upon 10 replications, making a structured sample of size $n = 120$. The design, duly replicated, enables us to put together a Q-sample (comparable with the ecological one for the earlier sample), but it now embodies Gorer's *theory* as postulated independencies. Statements to cover the design are taken from Gorer's book, e.g., as follows:

a d e. . . . . .The British Empire is iniquitous.
b c c. . . . . .The Chinese people are essentially like ourselves.
b c g. . . . . .American intellectuals, at meetings, have to prove boisterously to one another that they are just regular fellows.

It is a simple matter to put together 120 statements of this kind. Many experiments are then possible, directed toward testing Gorer's theory in pertinent conditions. A beginning might be made with a detailed study of a small-town businessman, a large-scale financier, a politician. There would be a *variate design* for each. Or "general theoretic" propositions may be asserted, e.g.:

Proposition 1: ΣC will be significant for some Americans, such that $c > g > f$.
Proposition 2: When ΣC is significant and $f > c$, $f > g$, then ΣB will also be significant, and $d > c$, and ΣA will also be significant, $a > b$.

The structured sample is wide open for factorial and factor studies of particular persons or families, or for appraisals in its terms in relation to cultural data such as films, novels, political speeches, and the like.

DESIGN FOR STUDIES ON ETHNOCENTRICITY

It is instructive to look at the methodology of the scales constructed by Levinson (103) on ethnocentric ideology. Theoretically, Levinson holds that ethnocentrism is based on a "pervasive and rigid ingroup-outgroup distinction," characterized by hostile attitudes toward outgroups and submissive attitudes within ingroups, with the corresponding views that ingroups are dominant and outgroups subordinate. Scales for attitudes about *patriotism*, *minorities*, *Negroes*, and *Jews* were constructed, and the fact that these correlated highly was used to support the view that ethnocentricism is of ideological proportions. Clearly, averaged nomothetic procedures are at issue; the validation of the scales, their norms, etc., are all R-

technique in principle. The scales concern only one independency or effect at a few levels, namely, *ideology*, with *patriot*, *minority*, *Negro*, and *Jew* levels; these are concerned, moreover, with *content* rather than with *attitudes* as such, which are nowhere directly represented. The attitudes, as behavior, concern the supposed hostilities, submission, dominance, and the like. We seek to represent both content and attitudes in a Q-sample, and the very simple design in Table 2

TABLE 2

Independencies	Levels				No.	D.F.
A, attitude (1)	(*a*) Hostile	(*b*) Submissive			2	1
B, attitude (2) ..	(*c*) Dominant	(*d*) Subordinate			2	1
C, ideology (content).........	(*e*) Patriotism	(*f*) Minorities	(*g*) Negroes	(*h*) Jews	4	3

suggests itself at once. The sixteen combinations for this design can be covered with statements similar to those used by Levinson, as in these examples:

a c g.......... Most Negroes would become officious, overbearing, and disagreeable if not kept in place.
b c e.......... I put complete loyalty to my country high above all other considerations.
b d e.......... I just *love* my dear, *dear* country.

The latter two examples are provided as an indication that statements are possible for all combinations of the design, since it might be thought that hostility-subordination and submission-dominance are incompatible combinations. On the contrary, all possible combinations "make sense," and the design serves to draw attention precisely to just such entailments which might otherwise be overlooked; it is, indeed, a well-known failing in implicit forms of theorizing that some of the logical possibilities are overlooked, a matter to which Woodger (205) and others have drawn attention in relation to theory construction. Thus the arrangement *b c e* is a combination of submission to an ingroup and dominant attitude or aggressiveness, whereas *b d e* suggests complete submission and subordination. An example of hostility-subordination would be as follows:

a d h.......... Who rules us all, at bottom? The Jews.

Our expectancy is that the ingroup-outgroup hypothesis, widely prevalent in group dynamics, will be explored along Q-technique lines. Experimental work under role-playing conditions can be used to test the dynamic theories at issue. Meanwhile, a door has been pushed slightly ajar, opening the way to a region in which attitudes can be experimentally investigated.

THE METHODOLOGY OF ATTITUDES

Attitudes are merely modes of behavior. But it is interesting to see what two leading social psychologists, Krech and Crutchfield (100), have to say about them. Their concern is with their *measurement* and, therefore, with the question of their dimensions and attributes. It has long been held, they remind us, that only *common* beliefs and attitudes (held by numbers of people) are susceptible to measurement and that those held by *single* individuals are not (100, p. 209). Krech and Crutchfield doubt this; many important aspects of an attitude, they think, such as direction, intensity, importance, etc., "seemed capable of being characterized in an absolute manner for an individual," and these did not seem to depend upon *norms*. But the authors do not provide *operations* for these supposed "absolute characters." Nor are they the consequences of any. No scientific formulation is at issue. The situation is very different in Q-technique. Thus, with respect to *importance*, we can ask members of our family in the "things American" studies to assess the Q-sample for the following condition of instruction:

a) What is the relative *importance* of the statements, with regard to relationships between peoples?

Or, for *direction*, we may give the following instructions:

b) If things go on as they are, what do you think the situation will be like 10 years from now?

That is, there are *operations* (the respective Q-sorts) corresponding to the conditions of instruction. We do not assert that these particular conditions at *a* and *b* are apposite to what Krech and Crutchfield had in mind as "dimensions," but it is certain that singular Q-technique operations of some kind can be conjured up for any testable propositions that these authors may assert, in the belief that such dimensions as *importance*, *intensity*, *direction*, etc., are at issue. The supposed dimensions, with any "general implications" are quite

unnecessary. Thus Krech and Crutchfield were correct in supposing that *norms* are not necessary, because nothing really exists about which to compose them. On the other hand, if the notions of *measurement*, *norms*, *scales*,[1] and all nomothetic conceptions related to these are put aside as postulated matters, experimental methodology is left wide open for the social psychologist's regard. He proceeds by asking questions which are intrinsic to concrete behavior. It will be said that, even so, attitude *scales* are needed, just as are intelligence tests. But technological or actuarial, rather than scientific, matters are then at issue, matters for pollsters, opinion surveyors, and the like.

PSYCHOANALYTIC THEORY OF PREJUDICE

It is a repeated objective in these chapters to illustrate how Q-technique can put to some proof one's hypotheses or theories, as direct testable matters. A set of studies by Adorno, Levinson, Sanford, and Frenkel-Brunswik (3) on the nature of the supposed *authoritarian* personality, provides us with an example of this kind. Frenkel-Brunswik, in particular, undertook a large-scale inquiry about the mechanisms at work which lead to the development of the highly prejudiced person.

Students were chosen who scored at the extremes of the Levinson Ethnocentrism Scale (103) and who, therefore, at the one extreme expressed highly prejudiced attitudes toward minority groups, Negroes, Jews, and the like. Frenkel-Brunswik wanted to know what sort of personality qualities these prejudiced persons had, relative to the nonprejudiced, and arranged for an elaborate series of interviews for them, carefully constructed so that different interviewers could pursue very similar lines in their interviews of the students. In this way, with great care for the details, protocols were made available for some 200 students. Frenkel-Brunswik collated these data herself and found circumstantial support for the following conclusions:

a) Prejudiced persons (as judged by the Levinson scale) tend to be stereotyped in their attitudes about their parents: their mothers are held to be "wonderful," the most "terrific" persons in the world, "amazing persons"; admirable qualities of calm intelligence, essential worth, and utter sincerity are attributed to the father.

1. Our "forced" frequency distributions are not *scales* in any R-technique sense. Each Q-sort is particular to the individual making it and involves no postulates about *individual differences*, absolute units, equal units, pure units, or anything of the kind.

b) Yet the interviews provided evidence that the homes of these prejudiced persons had been far from reasonable: the rod had ruled rather than soft affection and good sense.

c) Frenkel-Brunswik argued, and found data in the protocols to support the argument, that *a* was *idealization* and must be a cover for much aggression and hostility in the history of these students. Sexual impulses were probably much repressed. It is because of this that the students are prejudiced—they are merely projecting aggression and hostility upon others.

d) Moreover, it is not to be expected that a harsh upbringing, such as these prejudiced students appeared to have had, would be conducive to acceptance of the finer affections, the softer arts, and good music, for these are inward-turning (internalized) sensibilities; instead, the prejudiced students would externalize their aggression in a press for power and an urge for success of an unrealistic kind. Opportunism and little in the way of genuine friendships, passivity, repose, and inner harmony, or the like will be in evidence for them, all of which seemed indeed to characterize the prejudiced students, to judge by the data gathered massively from the interviews.

These conclusions may well be correct, and, indeed, we esteem Frenkel-Brunswik's work so highly that we have no doubts at all that something of the above kind will be found for prejudiced students. From the Q-technique standpoint, however, we would like to have testable evidence for it, and about single students, from *their* operations; as the conclusions stand, they are such as apply from the *external* standpoint, that is, as Frenkel-Brunswik interprets the facts. Instead of the elaborate interviewing, then, we would design a singular experiment for the student to perform, in terms of Q-sorts, from which to reach evidence of the above kind directly in factor terms.

We would proceed as follows. First, the *idealization* possibilities would be represented with *a*, *b*, and *c* in mind. Next, we would try to put *d* to some kind of test. For these two objectives, *two* samples of statements would be required. One would consist of statements of the kind that students make about their parents. The other would concern itself with attitudes about the sensibilities and the like referred to at *d*.

The statements provided in our good friend Elsa Frenkel-Brunswik's protocols would provide an abundant number of statements, gathered under very representative conditions, about parents. They would be of the following kind:

1. Mother amazes me, she's so good.
2. Mother is a really beautiful woman.

3. She is a wonderful person.
4. Father is calm, a quiet person.
5. Father is sincere, well liked.
6. People love him (father).
7. Mother gets too interested in fads.
8. She (mother) gives too much advice.
9. She is possessive.
10. I'm father's favorite.
11. My parents would do anything for me.
12. Father and I were (are) always doing things for each other.
13. Father is hot-tempered, aggressive, a sore trial.
14. Mother is a bit of a bore.
15. Mother never seems quite to make a good showing.

. . . and so on.

A structured sample would be composed from these (Table 3). Six statements will cover this in balanced design, and, for 10 of each

TABLE 3

Independencies	Levels			No.	D.F.
A, parent	(i) Father	(ii) Mother		2	1
B, mechanism	(iii) Idealization	(iv) Reality	(v) Doubtful	3	2

kind, a sample of 60 is quite readily constructed. Thus No. 7 above would cover the combination ii and iv; No. 1, ii and iii; and so on.

The second sample, to represent the argument used by Frenkel-Brunswik at *d* above, would be simply structured, for equal numbers of statements concerned with the finer affections, music, poetry, friendships, ambition for power, ideals about success, motivation, and the like. Typical statements would be as follows:

1. For my part, I feel I'm a pretty good fellow.
2. I am always suspicious of people's motives.
3. I simply don't like reading poetry—never have.
4. I feel that other people have not counted for much in my life.
5. It's natural for me to see both sides of a question.
6. I don't have too much respect for most people's opinions. They're often so stupid that it's pretty hard not showing just how you feel about it.
7. Give me soft music, any time.
8. My disposition is such that I am hurt deeply by unnecessary harshness.
9. I try to get my way regardless of others.
10. I am happiest with my own thoughts.

. . . and so on.

Now we could proceed to our Q-studies. We might begin with, say, two students who score high for prejudice on the Levinson scale and two who score low. With respect to the parental sample, they could each provide Q-sortings under each of the following conditions of instruction:

i) Give a description of your parents, as they are to you now.
ii) What, do you think, do your parents themselves think they are like?
iii) Describe the parents you would most like to have, i.e., *ideally*.
iv) What do you think Negro parents are like?
v) What do you think Jewish parents are like?
vi) Give a description of your parents as they used to seem to be to you when you were, say, twelve or fourteen years old.
vii) At their *worst*, what were your parents like?
viii) If you were married, what kind of parents would you want you and your wife (or husband) to be?
ix) Imagine *dreaming* about your parents and describe the outcome.
x) If your father had been much older than your mother (or much younger if he is, in fact, much older), it might have made a difference to them: describe what the situation might have been under such circumstances.

. . . and so on.

We have said little or nothing throughout these chapters about the high degree of conscious effort required on the part of the experimental subjects for Q-throwings of the above kinds. It will be obvious, it is hoped, that the truth or falsehood of these sortings is in no way at issue—it will not matter to us whether the subject "tells the truth," "doesn't cheat," or the like. For whatever he does is open to our full factorial regard, and, indeed we need believe nothing but our interpretations of the factors that result or the effects that prove to be significant, and these could, of course, merely lead to the conclusion that X wasn't "telling the truth" or the like. The whole purpose is to offer opportunities for the subject to give himself away, by projection, rationalization, identification, idealization, and the rest; and that is why we probe into him, so to speak, by way of several variates such as i to x above. It would be difficult for him to pull wool over our eyes in a consistent fashion in all such throwings, and, in point of fact, with care about rapport and the like, richly diversified data can be obtained from a single person along the above lines.

Thus, proceeding as above, there would be a (10 $\times$ 10) correlation table for each of the four experimental subjects. Each would be

factored separately, and the factors interpreted. The variance and factor analysis would be oriented toward some proof or demonstration of the Frenkel-Brunswik hypothesis for each of the four cases, in the appropriate directions. We could confirm, in the sense of testing the singular propositions, that the prejudiced students were, or were not, subject to idealization, and the unprejudiced ones not, or the reverse.

With respect to the second of the samples concerning the various sensibilities, each student would make the following Q-sortings in terms of it:

a) Give an account of yourself as you are now.
b) Give an account of what you would like your best friend to be like.
c) Give an account of your father in terms of these statements.
d) Give an account of your mother in terms of the statements.
e) How would you like your best girl (boy) to think of you?

In this case, too, the variates could be extended for each subject to at least 10, so that a (10 × 10) correlation table would be available for each. Or a (20 × 20) table could be put together for the above five variates for the four experimental subjects. Factor analysis would then be directed toward some proof that the two prejudiced and the two unprejudiced differ in *factors*, in the direction expected for them. That is, they would be compared in terms of factor patterns.

It would take us into too much detail to describe an actual experiment along the above lines—that must be for another occasion. It is sufficient to show, again, that we can penetrate into matters with single individuals as our subjects and that, indeed, their study under conditions of gross averaging or the like for the sake of reaching statistical significance is entirely unnecessary and essentially foreign to, and destructive of, any sound scientific methodology.

BOCK'S BEHAVIOR CORRELATIONS

Most of the applications of Q-technique with which we have been concerned deal with self-appraisals or "subjective" appraisals about others. But we have seen that behavior can be observed "from the outside": the *frequency* with which particular units of behavior occur can be recorded and afterward correlated as a Q-technique matter. Thus, in one of our early studies (161), children were observed as they performed at the Q-boards, with respect to a list of perform-

ances that any observer could discern. Similarly, if children are in a playground, an account can be kept of each child, if need be, to note the frequency with which it is talking, sitting, quarreling, listening, fidgeting, and the like. A general discussion and theoretical development of this "external frame of reference" in relation to Q-technique was the subject of Bock's work (28).

Bock has elaborated upon the early formulations by suggesting that each child of a small primary group of M children can be observed, and a record kept at *minute* intervals, say, noting whether it is, or is not, engaged in conversation (that is, either talking or listening). The Q-sample is a series of successive minutes of observation. The data for the M children are correlated (tetrachoric coefficients) and factored. Along such lines it is possible to distinguish between subgroups of children within the primary group, since each factor will represent such children who are in conversation at much the same moments of time and, therefore, presumably are in interaction. Thus factors would appear in this way which distinguish between the boys and girls of a primary group, if the girls stopped talking as a teacher entered the room, whereas the boys did not do so.

Theoretically there are many interesting matters in Bock's formulations. Thus, if *behavior correlations* are at issue, the person who is most highly loaded with a particular factor, that is, who correlates more highly with all other members of his subgroup than does another single person, may have set the pattern of behavior and be the "real" leader of the subgroup. His behavior has probably been most imitated, and in this sense he led the subgroup: whether this empirical result agrees with other notions about the structure of the subgroup would be a matter for inquiry. The same logic will apply to applications of Q-technique to samples of statements concerning leadership. Again, the factor pattern in relation to a primary group could show how each member's interaction is distributed within it—into how many cliques he can enter, for example, since each factor would represent a clique or the like. Orthogonal factors, similarly, could represent "cleavage" between subgroups, and factions clearly have their place in the same technique. The discovery of the bases for choosing helpmates, whether "psychotelic" or "sociotelic" (Jennings, 86), that is, based on *feelings* of liking or upon

the practical needs of the situation, can be pursued along these behavior-correlation lines, as, indeed, they can be along Q-lines generally. The importance of Bock's work obviously lies in the attempt he would make to base his studies upon behavior as observed from the outside frame of reference, that is, our representative probe *b* (i). The whole of his stimulating treatment is a further example of the use of Q-technique, or of analogous procedures, for the probe *b* (i), and we would be the last to deny importance to studies of this kind, which, in related directions, Cattell has been foremost in exploring.

Among demographers and ecologists developments along these factorial lines could have an important yield, and it is certain that studies of the kind will multiply as the methods of variance design and factorial analysis gain wider recognition. Degan's (58) study of the legal decisions made by the Supreme Court of Justice and others that are under way are straws in the wind. With respect to social psychology, however, Dilthey would have thought that these *b* (i) applications of the correlation between persons ape too slavishly the natural sciences and that along these lines one can scarcely hope to reach into the bosom of inner experience. However, important information can be gathered along such lines.

OTHER REGIONS OF INVESTIGATION

Applications of Q-methodology have been fostered in several other regions of interest to social psychology, such as in role-playing (62), classroom situations (9, 61), fraternity house subgroups (27), clinical interactions (66), and the like. The study of leadership is readily open to us, in many different ways in Q-technique. Thompson and Nishimura (183) have studied whether *friendships* between persons are dependent upon a compatibility of *ideals* for the friends —each may regard his friend as possessing those personality qualities which he himself idealizes. A test of this hypothesis requires little more than a suitable Q-sample of statements and a few pertinent appraisals by mutual friends for variate designs comprised of such variates as (1) one's *self-appraisal*, (2) one's *ideal*, (3) an evaluation of one's *best friend*, (4) a description of someone heartily *disliked*, and (5) another about someone of whom one is *jealous*, and so on.

Studies on the effects of democratic and authoritarian forms of "atmosphere" or "climate" on children's groups constitute another area of importance for social psychology. Along similar lines a number of researches have been pursued with nondirective principles. The studies are often undertaken from the so-called "phenomenological" standpoint: how the individual behaves in a group is regarded as an exercise in cognition—the psychologist tries to study the "cognitive structure" (Krech and Crutchfield, 100). The method used is that of questioning the participants and "analyzing" their verbal reports, the analysis consisting of the personal selections of the experimenter or of counts of word frequencies and so on (74). The use of *open-end, intensive interviews* is highly regarded by Krech and Crutchfield (100), who state that no other technique seems even remotely to compare with it as a method for studying the cognitive structure of group participation, social beliefs, and the like. A thorough example of this kind is provided by the study of Smith, Bruner, and White (145) on the opinions and attitudes of 10 adults on things *Russian:* each adult was subjected to 30 hours of tests and interviews, including *open-end* and *stress* varieties. The tests included projectives, experimental social situations, and an information test on Russia as well as an opinion-poll questionnaire. Each subject also wrote a life-history. What we have said about Frenkel-Brunswik's study above applies to this, too, for having drawn their conclusions (which no doubt are very sound) these authors could offer no confirmation of the latter within the immediate context of their study. The reader by now can see how this can be done: the conclusions, if they are testable, as they should be, can be stated as a few *effects*, and operations upon these along Q-technique lines by the subjects themselves—taking perhaps half an hour at the conclusion of all the other testing and analysis of its data by the experimentalists—offers at once a way of summarizing the study as a whole and of yielding, on its own account, a salutary testing and proving ground for the researcher's conclusions. Intensive interviewing and testing for purposes of the kind that these authors had in mind should be regarded as incomplete until, at its end, it is rounded off with a Q-technique study to clinch the conclusions.

One of the earliest Q-technique studies of leadership was undertaken by Anderson (9) at the University of Chicago, in which some

classroom groups were investigated from the standpoints of "student-centered" and "leader-centered" structures.

An early study by Ebermann (61) in the socio-educational area provides an example of another aspect of Q-technique to which all too little attention has been given. It is true that we often make studies on very few persons, or upon only one; but this does not mean that the groundwork for some of these has not been comprehensive and thorough. Ebermann provides us with an excellent example of this: he was interested in investigating teacher-pupil interaction and had formed a theoretical view about the importance of *acceptance* and *rejection* in the classroom situation. Careful records were made of classroom interaction, for a total of some 70 teachers and upward of 200 observational sessions. From these records he compiled a large number of statements concerning behavior in these situations, related to such matters as aggression, dependency, sex, cliques, competitiveness, and the like. He had these statements categorized, before beginning any more detailed work, with respect to whether they denoted an accepting attitude or not on the part of the teacher or whether the meaning of the statement was doubtful or poorly worded, vague, or ambiguous in its meaning; in this way he could eliminate many statements and reserve for his future studies only those which seemed to be widely acceptable. (We are chary, in principle, of making such eliminations; it is often the statements about which there is no very settled consensus that prove to be the most sensitive for experimental purposes.) We cannot follow Ebermann through the details of his study here—it is reported elsewhere (61). But it provides a good example of the care that may go into the construction of even an unstructured Q-sample.

Several interesting investigations have been made to illustrate the possibility of studying the family and the marriage situation in Q-terms. The Child Development Project (University of Illinois), with its director, Dr. Kirke, is studying the conception that parents of defective children have about their children. Studies by Rosenthal (133) and Stock have begun the process of making explicit some of Bion's theorizing about group dynamics (26). Others by Fiedler and Blaisdell (66) and by Blaisdell (27) appear to have much promise for the study of small social groups or, rather, for social interactions. The general thesis is that the way persons X and Y interact is dependent

upon their respective attitudes toward each other, these being often of an unconscious nature. Blaisdell (27) made a factor study, perhaps the first of its kind, for a small structured group of friends. Using a simple sociometric device, he was able to pick out for factor study a small subgroup of fraternity house members, five men, four of whom (A, B, C, D) had expressed great liking for the fifth (E). The sociometry, so to speak, is as indicated in Figure 1. As a basis for Q-descriptions, Blaisdell made use of a sample of statements put together originally by Fiedler and drawn, we believe, largely from Murray's work on personality.

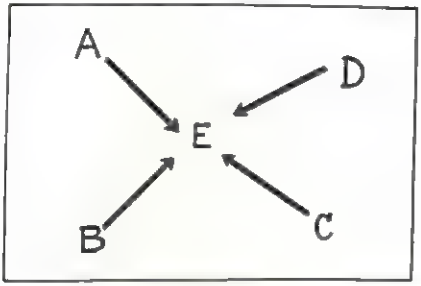

FIG. 1

In terms of these statements, each of the five men was called upon to give the following descriptions:

a) Describe yourself as you are *usually*.
b) Describe your *ideal*.
c) Describe the friend you like *best*.
d) Describe the friend you like *least*.

A (20 × 20) table of correlations results, which, when factored by centroid method, gives the four factors of Table 4. For simplicity, we shall deal with the first three factors only, which we rotate to provide I′, II′, III′, by paying special attention to E's descriptions, and to those of *c* and *d* for the four men who are so attached to him. The main points to note about this solution are as follows:

Factor I′ represents E's idealized self, orthogonal to a projected self-notion he has of a best-liked versus least-liked person, which is factor II′. That is, what E thinks of himself, closely related to the ideal he has for himself, is uncorrelated with the kind of personality he appears to like or dislike, at the other pole, in others (the matter is clearly shown in Fig. 2).

Factor III′ (and we suspect the same for IV) concerns the personality of two of the men, A and C, orthogonal to E, and uncorrelated with E's idealized self-factor I″.

If we look at Figure 2, which shows the plot for the factors I′ and II′, the following interesting results appear: three of the men, B, C, and D, placed their "best-liked" description on factor I′, that is, corresponding to E's own idealized self. A, however, places his

TABLE 4

BLAISDELL'S DATA; n = 76 STATEMENTS

			Blaisdell Unrotated Centroid Factors				Rotated Factors (Stephenson)			"Types"		
			I	II	III	IV	I′	II′	III′			
E:	1	Self	28	17	−29	29	43	01	04	α	...	.
	2	Ideal	49	17	−32	24	58	−10	17	α	..	.
	3	Best-liked	−16	44	14	18	−04	46	15	...	β	. .
	4	Least-liked	31	−40	15	−32	−07	−50	14	...	−β	.
A:	5	Self	76	−39	40	14	01	−72	59	...	−β	γ
	6	Ideal	77	−32	47	27	−01	−67	68	...	−β	γ
	7	Best-liked	62	−13	20	41	16	−43	47	...	−β	γ
	8	Least-liked	−05	18	31	26	−19	18	23	...	...	..
B:	9	Self	50	26	−21	−22	53	−03	27	α	...	.
	10	Ideal	10	44	18	−32	07	33	34	...	β	. .
	11	Best-liked	56	13	−21	12	52	−18	27	α	.	
	12	Least-liked	−26	24	40	−21	−36	34	18	−α	β	.
C:	13	Self	39	57	42	−19	11	29	74	...	(β)	γ
	14	Ideal	40	57	39	−25	13	28	73	.	(β)	γ
	15	Best-liked	42	19	−12	29	39	−05	26	α	. .	.
	16	Least-liked	22	20	22	29	03	06	36	...	...	. .
D:	17	Self	21	−22	−04	−27	08	−29	02	...	(−β)	.
	18	Ideal	72	−31	−13	−10	41	−63	25	α	−β	.
	19	Best-liked	36	12	−13	−27	35	−08	18	α	.	.
	20	Least-liked	33	−20	−21	10	27	−34	−02	...	−β	.

best-liked along the negative end of factor II′, that is, at the kind of personality that E dislikes most! We may suppose, then, that there is some evidence, with regard to the men B, C, and D, that they regard E as E regards himself and that this supports the proposition that the same mutual attitudes in this respect will make for compatibility and friendship. But the man A happens to regard himself as on the least-liked end of factor II′, that is, he is precisely the sort of person that E says he most dislikes. But, similarly, D's *ideal* self

is a mixture of E's ideal self, and the negative end again of factor II′, that is, a certain ambivalence is indicated. In the latter two cases we may suspect a considerable element of distortion in the regard that A and D have for E. This is shown as clearly for the typification columns of Table 4, for α, β, and γ. The men B, C, and D all "hit upon" factor I′, that is, type α, either positively or negatively for

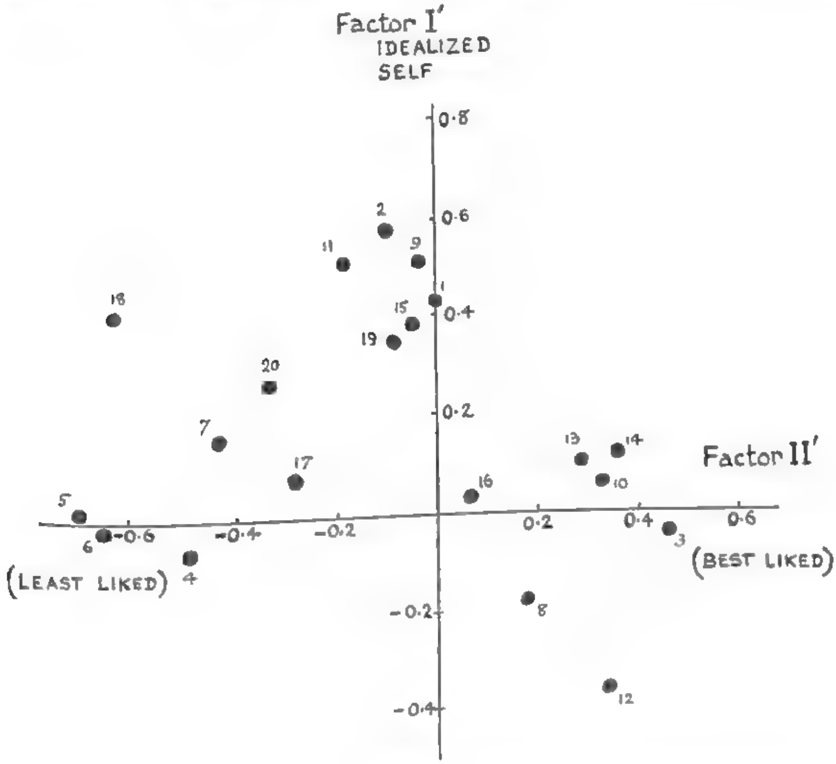

FIG. 2

one or another of their positive appraisals (self, ideal, or best-liked). Factor II′ is also remarkably discriminative, A and D being negative for it, and B and C positive. Factor III′ is indicated for A and C. Thus, whatever the explanation may be, factor I′ is indicative of some kind of compatibility of attitudes which comport with those of E himself; but factor II′ is undoubtedly some other aspect of E, the negative part of which is grasped by A and D, and the positive by B and C. Yet we may suspect that none of these men are "really" like what they here believe they are like. The argument, so far, con-

cerns the factors and their loadings only; but we can turn to the respective factor-arrays for I′, II′, and III′, and see what it is that is so distorted, and, probably from related sources, why. We believe that it is in studies of this kind, for subgroups (determined perhaps by sociometric indices of some kind) within primary groups, that the matters of the greatest interest lie for the study of human compatibilities, friendship, leadership, and the like along these Q-technique lines.

CONCLUSION

Studies of attitudes, beliefs, religious faith, nationalism, and the like are clearly open to our regard along Q-lines from a subjective standpoint. The study of small social groups is as obviously facilitated, whether from "internal" or from "external" standpoints. There can be no boundaries between anthropology, social psychology, and subjectivity in the study of man's social behavior. Only the barest beginnings have been made in looking at the latter in operational terms, directed, as we would have it, by theoretical considerations. Our main concern has been to illustrate the possibilities, and a formal account of social psychology in relation to Q-methodology must wait for a later occasion.

Meanwhile, a number of details are worth mention. The Frenkel-Brunswik study discussed above provides an illustration of the method we frequently use or recommend for putting conclusions from large-scale studies to a brief empirical test. It has wide applications. The psycho-sociological studies of Bettelheim and Janowitz on the *Dynamics of Prejudice* (24), and Ackerman and Jahoda's studies (2), as well as many others of the same carefully conducted and closely reasoned kind, become more significant if their conclusions can be put to simple proof along Q-technique lines. This should not be taken to mean that the latter can replace the broader basis of psycho-sociological study; but conclusions may leave us unconvinced, and a little supplementary evidence, of the kind that Q can provide, may be welcome.

A few words should be added, too, about the logic of attitude scales. The construction of such scales along Thurstone's lines (189) is well known, as is Lickert's (108) method. Thurstone makes use of the method of paired comparisons, and it is important to say how

these methods differ from the forced distributions employed in Q-technique. Thurstone begins, as we do, with sets of *statements*. He has these judged according to principles subsumed under the law of comparative judgment (185). The data so provided give a basis for *scaling* the statements in terms of normal probability distributions. Under certain conditions, statements can be chosen which differ by equal amounts from one another in the probability of their choice by judges; in short, a scale can be composed along which consecutive statements differ by equal discriminative intervals. Given m statements, they can be placed in rank order in terms of the law of comparative judgment. These procedures, however, are in no way comparable to those of Q-technique, for the following reasons: *first*, "general implications" are involved as postulates in the use of such *scales*, and not in Q-sortings; *second*, the foundations of a Q-sort are laid in the *principle of randomization*, with precise specification of the conditions for estimating error and with tests for homogeneity; *third*, many different effects may be specified in a structured Q-sample, and this is never involved in *scales*, which, on the contrary, are meant to be "pure" or the like with respect to a single attribute. Lickert's method of scale construction appears to give much the same result as Thurstone's and is as obviously rooted in the principles and practices of individual differences. Naturally, large-scale testing has many important practical and technical applications, and, for these, properly constructed scales are essential. A Q-sort, however, has no such pretensions but relies, instead, upon small-sample theory and Fisher's experimental methodology.

CHAPTER XI

Q-METHODOLOGY AND SELF-PSYCHOLOGY

OBJECT

THE self is everywhere implied in common-sense psychology. But it is more likely, among psychologists, to be suspected for its ghostlike or spiritual qualities. Prior to modern positivistic psychology, it was reasonable enough to regard the self either as a rubric for the contents of a mind or as an integrative principle of some kind. Thus man's emotions, desires, wishes, and intentions, such as belong to his so-called "inner experience," were believed to be given by immediate experience and were thought of as the content of a mind or a self. But if a *feeling* can be "given" as immediate experience, so might a substantive self itself be grasped—at least in principle; and even in contemporary times psychologists have reported that they have caught at least a fleeting glimpse (150) of such a self, given in immediate awareness. It will be apparent that our concern is to be with no such fruits of introspection, whether it be the naked self of awareness or the self as a mere rubric or integrated stock of all one's immediate experiences. Instead, we propose to begin the study of self-psychology from the mere standpoint of what a person says about himself, what he believes he is like. Aveling (11), who caught a glimpse of the self of awareness, also saw, but not so clearly, that man has notions about himself, about how he has acted in the past, and about how he might act in the future. It is these notions that we propose to study as the essential subject matter of self-psychology. We find that it is easy to reduce these to operations along Q-lines.

A very brief historical reference is helpful, before we begin with these Q-technique applications. Cicero, a very long time ago (106–43 B.C.), provided us with a text of concern to us in these chapters. He distinguished between five areas of interest to students of human conduct—there are, he said,

a) What you think of yourself.
b) What others think of you.
c) The part or role you play in life.
d) A special selfhood is sometimes reached, of great distinction and dignity perhaps, that may characterize a person and lift him above the common mass of *a, b,* or *c.*
e) There is the vast assemblage of personal qualities that constitute a man's capabilities or potentialities, which, as Cicero put it, "fit a man for his work."

It will be obvious to the reader, by now, that *e* is the concern of R-methodology. We propose that *a–d* are as certainly the concern of Q. The early behaviorists rejected *a–d* as of no scientific interest, either because of the intrinsic subjectivity at issue or because of the gross unreliability of such notions; for who, it was argued, could possibly believe what a person cares to say about himself? Nowadays, of course, we do not necessarily *believe* him; we have learned to look behind what he says and to see consistencies where none were apparent before. This is what is dealt with, essentially, by the *projective* test techniques (118). We have a similar contribution to offer, namely, to the effect that a person's *self-notions* can be studied in operational terms, to see what consistencies underlie them, if any.

MODERN SELF-PSYCHOLOGY

Thus we are not to be concerned in any direct way with much that is discussed today as ego theory. Logical analysis of *ego* concepts must be reserved for a later occasion. Our connections are much closer to the recent growth of American self-psychology, as represented by the work of Angyal (10), Lecky (102), Rogers (129), Snygg and Combs (147), and others. We do not, however, accept a phenomenological theory, as these workers have done. Thus Snygg and Combs (147), in a recent formulation of the principles upon which they suggest self-psychology should be based, posit the usual phenomenal field, which can never have an existence independent of the person experiencing it, which is never open to direct observation, and yet which is the cause of behavior. These fields are supposedly experienced at *instants* of action, and the psychologist's job is to reconstruct them by inference. There is a one-to-one relationship between the person's phenomenal field and his outward behavior, the latter, of course, being public. All such postulates, we believe, are quite unnecessary. What is at issue, it seems to us, is the

fact that each of us can reflect and make reference to himself: life has in it many occasions when we think about ourselves. We wonder about *our* unworthiness at church, *our* grip of things at business, *our* hopes and aspirations. These matters can be studied without phenomenological speculations.[1]

It is argued (147) by phenomenologists, for example, that, whereas individual behavior cannot be predicted according to normative (R-methodology) principles, neither can predictions be made about particular self-referent statements. The person, however, carries about with him, so to speak, some "conceptual roles" (Snygg and Combs) which are stable characteristics and about which predictions of behavior are possible. Thus the student may conceive of himself as scholarly or as having a flair for the fair sex; and each of us may have several such roles that we play in appropriate behavioral settings. The concept is analogous to that of *sentiments*, as developed by McDougall (113). Clearly, these are acquired. Once learned, they persist, often out of all keeping with the immediate needs of a situation. They influence behavior. Thus the individual cannot behave entirely independently of the way he thinks of himself—if he thinks of himself as gentle, he will behave like a lamb. These various roles are not acquired with equal ease: the soldier cannot be frankly a coward but may readily develop the role of a buddy. It is argued that contradictions between self-notions, or the roles subsuming them, do not remain conscious—some are suppressed. The self-psychologists also adumbrate a more general self-concept that the subject may carry about with him, so to speak. The various conceptual "roles" are in some way integrated, to provide a self-consistent self-picture.

Similarly, when an individual is totally incapable of acting in accordance with these roles, there is no self-concept, no self therefore, as in schizophrenia; or, contrariwise, the person inflates his self-concept into the self-aggrandizement and delusions of a paranoiac. So Hitler, we may suppose, had a consuming passion to realize the self-concept that fate had fashioned for him, as a man of destiny. Certainly, the man who is insecure or in need of psycho-

1. Even as matters of general psychological concern, indeed, the American self-psychologists seem to have bet upon the wrong horse in the psychological race. The Gestalt principles, upon which they based their heavy odds, are beginning to dissolve under the critical eye of physiological psychology, for example, as in Hebb's (78) notable work.

therapy is likely to be preoccupied with himself in this conceptual sense. His is an unhappy, bewildered self-concept. Psychotherapy, therefore, becomes a matter of changing the person's self-concept, so as to bring about readjustments in his behavior because of a reorganized self-concept. It is certain that some people seek to *understand* themselves, having an "urge to grow." It is true, too, that vanity, pride, and self-importance may be all-intrusive in conduct. The preacher in Thomas Hardy's *In Church*, it may be recalled, glides into the vestry after his service, and thinks he is alone there: but the door swings softly open,

> And a pupil of his in the Bible class,
> Who adores him as one without gloss or guile
> Sees her idol stand with a satisfied smile
> And re-enact at the vestry-glass
> Each pulpit gesture in deft dumb-show
> That had moved the congregation so.

It is not difficult to believe that such a man's self-concept was his guiding force, holding the reins of all his conduct. But history and everyday life alike provide instances of the opposite order of things, of utter selflessness, for men and women who seem to lose all sense of self in the pursuit of fortune, duty, or devotion.

The self-psychologists are to be congratulated, however, upon reviving an interest in the self. They have objected, too, to the excessive scientific claims made for normative methods in psychology, which we call R in these chapters. The traditional way of dealing with behavior, they aver, is to ignore all but what happens "on the average." More fundamentally, long before them, James Ward (197) had maintained that the measurement of man's attributes, the assemblage of his capabilities, was "psychology without a subject" or "more exactly a psychology which ignores the *subject* that it everywhere implies." His criticisms of the so-called "objective" approach were trenchant at a time when none would believe him. Naturally, we, too, find the normative methods of individual psychology unsound. But that is not to say that we can dispense with statistical methods; on the contrary, they are clearly essential in their proper place.

The self-concept is often spoken of as essentially mind (147). Usually it is also a Gestalt, a whole, a "oneness" and unity of the

many roles upon which it is based. One may wonder what this means in relation to phenomenology. For all that can conceivably be phenomenological is a specific and particular *instant* of perceiving. These conceptual roles, however, and the self-concept, are essentially classes of behavior, grasped by the *psychologist*, or a theory about behavior—the psychologist's theory. It presumes the internal framework. But it remains external to the person. Thus it is essentially a matter of *a* (iii) of our representative probes (p. 96), at most, with some leaning toward *a* (i) as well. Yet the theory is such that it can be dealt with, instead, by way of probe *a* (ii), and this, we are to propose, is the valid domain of self-psychology.

The point is not a fine scruple, nor does it merely refer to our rejection of phenomenology or of experience as such. Methodologically, the self-psychologists have *classified* behavior, as into conceptual roles. They thereupon seek to theorize about these collectivistic matters, as though they existed as such concretely. The same is true of the self-concept. They pursue the formula of first asserting some *postulates* (Snygg and Combs, 147; Rogers, 129), and then argue from this in a deductive manner, to derive consequences which can be put to test in the hypothetico-deductive fashion. Thus one may deduce that a client will utter more personal pronouns early in psychotherapy than toward its completion (Grummon, 74). One then merely makes enumerations in suitable protocols, to see how frequently the patient utters "I", and the proposition is put to empirical test. The inner reflection, as such, of the patient is thus never operated upon *directly;* something is always being inferred from postulates and tested objectively. We regard this as essentially our probe *a* (iii), because, by such means, one's operations never touch upon the person's self-conceptions as such: the *psychologist's* postulates, his theory, the deductions therefrom, and the actual operations (e.g., counting the number of times X says "I"), all serve to keep the psychologist at a certain distance from the subject's concrete subjectivity. Our own approach is to plunge directly into the latter. This means that we seek to *discover*, in the first place, from the individual's own operations, what, if any, are his modes of subjective behavior. We assume no collectivistic frame of reference about roles or classes of behavior. Nor do we require postulates of the kind asserted, perforce, by Snygg and Combs (147), Rogers

(129), Sarbin (137), and others. (Where, indeed, these postulates are not phenomenological and therefore spurious they have the status, in our methodology, of *abductions*,[2] or of general hypotheses, rather than of specific postulates of a scientific system.)

THE EMPIRICAL STUDY OF SELF-NOTIONS

We make a beginning in this operational definition of a person's subjectivity by attending to his self-referent statements and self-notions. These are statements a person makes about himself, with reference to his personality and interaction with others, as in a diary, journal, or autobiography or in the course of talks, interviews, and the like. All have reference to himself as a self in action, reflection, retrospection, or the like, as more or less conscious matters; or they are statements he makes about *others* which might be projections of such self-notions and are therefore to be regarded as non-conscious notions. The latter can be gathered from TAT tests, where the search is for functional projections. Clearly, such self-notions, whether they are statements made with full awareness of self or statements made when the subject is not being "self-conscious" or made in retrospection or during free association, or in the course of responding to a TAT situation, have reference to the person in his behavioral milieu. A college student is likely to make reference, in these self-notions, to his home situation, his academic life, his interaction with friends, his special stresses, hopes, aspirations, unhappiness, and the like. The statements are likely to be unique and certainly highly particular to a person; the real meaning of many of them could be buried in fantasy. It is with such statements, *gathered in natural settings as far as possible* (or in careful retrospections or the like), that Q-technique begins its study of the self. Two of our graduate students, Edelson and Jones (62) spent several weeks collecting suitable statements from their experimental subject, making use, for this purpose, of complete recordings of interviews. The subject kept a daily journal, in which he entered his reflections for the day, and this, for a sophisticated Chicago student, was a rich source of self-referent statements. Statements are then abstracted and edited. Short state-

2. Thus one of Rogers' postulations is that "most of the ways of behaving which are adopted by the organism are those which are consistent with the concept of self" (130); this is a general theoretic proposition for us, leading directly to many singular testable propositions along Q-lines.

ments, each as far as possible for a single idea, are better for practical reasons than long or complicated sentences, and, since the subject has made the statements himself, they are likely to have a fairly stable meaning for him. The following are typical statements of the kind we have in mind, taken from Edelson and Jones (62):

1. I am usually a composed person.
2. Sometimes I feel lost and abandoned.
3. I care about influencing others so that they'll do what I want them to do.
4. When I get a woman in my arms, I get the idea "This woman is mine, all mine. You've conquered this woman. You've done well."
5. I believe that in the long run one must submit to fate.
6. I believe that religion is a crutch to man. It gives him something to depend upon. A man who acquires a deep belief in himself discards religion.
7. Some of the things I do aren't exactly ethical.
8. I don't like to be alone with myself. I'd go mad living by myself all the time.
9. Power is a great feeling. I glory in it!
10. The main thing in my life is a lust for good things. I like nice clothes; I like beautiful women. I like good food. I like luxury. Practical luxuries.

It is clear that the subject's behavior is spread out as well in panoramic fashion for our psychological regard, when several hundreds of these statements are placed before us.

Our purpose is not to study these self-notions as such but to use them as the raw material for the development of a self-psychology. The concern is with "verbal report" in the widest sense, with retrospections, self-reflections, free associations, fantasy, and the like of a person. All is behavior of a self-referent kind. That rich sources of psychology are involved, such as psychoanalysis has tapped, is surely plain for all but the most prejudiced to see. But it is as complex as it is subjective, and physiological and "objective" psychologists will attend to it, if at all, with the suspicion of a tax-collector going through returns. The word "subjective," however, has two meanings for us: in the one it is contrasted with "objective" and means "unscientific," and in the other it merely has reference to the obvious fact that we have self-referent notions. Even the strictest behaviorists, such as Watson, Hunter, or Skinner, did not deny importance to the latter *subjectivity:* the difficulty was merely that of finding "objective," that is, dependable, *operations* for it. Up to now the only serious attempts to explore it have come from the psychoanalysts. In the method of free association Freud dealt with a vast tissue of subjectivity under the condition that an uncritical

attitude be adopted by the subject; and all psychoanalysis and *dynamic* psychology is testimony to the rich results of its exploration. There are similar gold mines to be exploited, we suspect, when other attitudes are adopted and when techniques other than free association are employed for their study. Moreover, to this day the psychoanalytic doctrine has remained exploratory only. It has drawn attention, fundamentally, to many interesting classes of behavior, subsumed under such terms as "projection," "repression," and the like, but it has given rise to no experimental methodology particular to its own needs. None of its complicated "interpretations" is ever given an empirical proof, of the kind that scientists look for and expect. It is true that some psychologists, notably Sears (141), have tried to test the doctrine, but they proceed from the false premise that the doctrine can be tested with respect to "general implications," in terms of individual differences. What is required, instead, is an experimental procedure for the singular and concrete individual behaviors, and along Q-technique lines we can achieve this. We now know, therefore, how Freud could have put any of his hypotheses to test for each or any of his patients, and, by the same token, we can now experiment objectively with *subjectivity* of the self-referent kind, in general.

AN EXAMPLE FROM PSYCHOANALYSIS

It is impossible to provide the prototype of all experimental work now open to us in this way. A beginning can be made, however, by taking an example from psychoanalysis, from Freud himself, for his early patient, Dora, whose abortive analysis is described in his famous "Fragment of an Analysis of a Case of Hysteria" (71). It is a simple matter to represent Freud's hypotheses about this case in a Fisherian balanced block design.

Freud had surveyed and explored Dora's free associations (and, no doubt, many self-reflections and periods of rumination besides) for three months. The retrospective "report" covered Dora's later adolescence, and the crucial behavior at issue was summed up, as often happens, by two of Dora's dreams. From the analysis as a whole, Freud drew certain conclusions, and it is our object to show how these could have been empirically tested, in terms of operations performed by Dora herself, from her own self-notions.

REPRESENTING THE INDEPENDENCIES

The balanced design which represents Freud's hypothesis is required. It will be recalled that the case involves Dora's rich, hypochondriacal father, a stupid mother, and Herr and Frau K. Father and Frau K solaced each other, and Dora and Herr K became entangled in similar affections. Freud uses the analysis of Dora's dreams to represent his hypothesis about her conflicts. Dora had had a lifelong history of hysteria (a neurotic repudiation of sexuality). But the particular breakdown and distress under analysis were instigated by overtures to Dora by Herr K, from which she revolted. The conflict situation, from which the dreams emerged, was an epitome of her recent history. On the one side there was a desire to

TABLE 1

BALANCED DESIGN FOR THE CASE OF DORA

Independencies	Levels	No.	D.F.
A, persons . . .	(*a*) Father (*b*) Herr K (*c*) Frau K	3	2
B, conflict	(*d*) Yielding (*e*) Respectability (*f*) Neurotic response (*g*) Revenge and hostility	4	3

yield to Herr K, but opposed to it were her sublimatory feelings of respectability, her good sense, as well as the neurotic repudiation of sexuality; interwoven hostility, occasioned by a maid's disclosures of Herr K's promiscuity, was also involved, as well as wounded pride and, as a consequence, a deep motive for revenge, which stemmed remorselessly (according to psychoanalytical observations) from homosexual sources attending Dora's relations with Frau K. Here, then, is the typical psychoanalytical kettle of fish.

We represent the argument by two main effects, one, *A*, concerned with the *interactional persons* at issue (her father, Herr and Frau K, in particular), and the other, *B*, the *conflict situation* (her neuroticism, her love for Herr K, and so forth). A balanced design is shown in Table 1 for the major effects and their levels. Each effect could have additional levels or degrees of freedom—Dora's mother, for example, or her boy friend, might be included at *A*. The effect *B* represents the conflict, with *d* on the one side and *e*, *f*, and *g* on the other. The design represents the situation *as Freud saw it*. It is

his hypothesis about the case. Needless to say, psychoanalysis is full of such hypotheses, arguments, and conclusions about cases, and it is always possible to represent them in such balanced designs.

The trouble in the past has been to put such conclusions or "interpretations" to independent empirical test for their proof or disproof. There are, in fact, many ways of achieving this, bearing in mind the need to make the *case* at issue the center of operations. If Dora, for example, could have been got to respond to a suitable set of *thematic apperception pictures* (118), some evidence of Freud's hypothesis about her might have been reached independently. However, TAT protocols, like dreams, have to be "interpreted" by the psychologist, so that the definitive operations are not the subject's, but the psychologist's. We achieve the required subjective operations along Q-technique lines.

The complete experimental procedure we have to consider involves, besides the balanced design, (i) a Q-sample derived from it; (ii) the assertion of propositions in relation to the experimental subject and the theory or hypothesis at issue; (iii) *operations* by the subject, directed toward providing the necessary evidence for ii; and, finally, (iv) dependent forms of analysis of the data so provided. In principle, any number of Q-samples can be composed for any one design, and there can be innumerable propositions for any subject, and likewise many *operations* under different conditions of instruction and experiment.

CONSTRUCTION OF A Q-SAMPLE

Dora is now an old lady, if she lives, and we cannot hope now to recapture her self-reflections of some sixty years ago, nor could we expect her to relive the interactional settings of her youth. But had we been with Freud at the time, we could have gathered the necessary, and sufficient, statements from her either as verbatim recordings of her conscious musings or as her responses to suitable *projective* material (in which she would undoubtedly have said things *for* persons in the pictures of a TAT, for example, as projections of her own preoccupations, which we might call "non-conscious self-referent statements"). Failing Dora, we have to turn elsewhere for examples of the kinds of statements we have in mind: we shall refer shortly to concrete cases like Dora herself. For our present

example, however, we went to George Eliot's novel *Daniel Deronda*, the central character in which is one Gwendoline, into whose mind Eliot must have projected difficulties not unlike Dora's, and a glance through the novel gives us a rich flood of the kind of self-reflections which, we are sure, Dora must have spoken many times over. They are of the following kind:

i) Things are insecure, false; I see them in the light that suits me best.
ii) I can be ill from longing.
iii) I can think only of father.
iv) I can be well from joy.
v) Physicians I spurn.
vi) Inwardly I am defiant.
vii) I tingle with resentment at being overlooked.
viii) My position is one of humiliating dependency.
ix) I have a tragic mouth.
x) I mean to please myself.
. . . and so on.

Now, in a rough-and-ready way, statements of this kind, gathered from Dora herself, must have touched upon her conflictual situation, in however tortuous a manner. And we can make them do so again when we invite her to perform Q-sorts with samples of the statements: herein lies the possibility of testing the concrete dynamics of the case, such as Freud was concerned to explain. For rough-and-ready purposes, again, we could have taken a random sample of Dora's statements, say 100 in all, and performed our experiments with them. But we can structure a sample for the hypothesis already represented in a balanced block design of Table 1.

This particular table for balanced design consists of the following combinations of effects:

$$\begin{matrix} a\,a\,a\,a & \quad b\,b\,b\,b & \quad c\,c\,c\,c \\ d\,e\,f\,g & \quad d\,e\,f\,g & \quad d\,e\,f\,g \end{matrix} \qquad (n = 12)$$

Thus the combination *ad* could be fitted by statement iii; statement viii could fit well into *bg;* and statement vii, unconsciously, into *bf*. We can easily put together in this way 12 statements to cover all the combinations once each, or *z* replications of each can be taken, making our sample of size 12*z*. In principle, any such sample would be adequate for our further purposes, although the perspicacity of the investigator can be used to put together statements which are more likely than others to be pregnant for dynamic purposes.

THE PROBLEM

The sample becomes a sort of proving ground of the theory at issue in the balanced design. The assumption is that the case can be got to perform or operate with these statements, as in Q-sorts, in such a way as to provide evidence of her own dynamic processes, and these will be such as Freud hypothesized for her case. The "general theoretic" proposition is to the effect that Dora's problem concerns hysterical, homosexual, sublimatory, and hostile mechanisms in special relationships. Evidence is required about these from Dora herself.

There could be innumerable singular propositions, however, for Freud's "general theoretic" one. The art of experimenting is to devise the testable singular propositions. One solution is to ask the subject to offer self-descriptions under different conditions of instruction, in such a way as to cause her to "give herself away" or to relive some of her stresses as projections, rationalizations, defense mechanisms, fixations, and all else of the kind. The questions would be of the following type: What does she [Dora] believe Herr K thinks of her now? And what does she think Frau K believes of her? What does she believe her father thinks she is like? What would she prefer to be like, *ideally?* What does she think Freud believes she is like? What, did she think, was Herr K's opinion about her when she was sixteen? Dora could have provided evidence in this way, to support Freud's hypothesis about her. For some conditions of instruction we could assert that levels *d* and *e* of Table 1 will be significantly greater than *f* and *g*. For others the situation would be the other way round. Most of these facts can be predicted with success; their testing is a matter of variance analysis in the customary Fisherian way of small-sample doctrine.

But we can go further. The dependency factor analysis for Dora's self-arrays could give rise to factors, the results of which could correspond completely with those for the prior variance analysis of the arrays, according to the principles outlined in chapter vi. The two effects *A* and *B* of Table 1 would provide an orthogonal cleavage; but the levels within each could also give rise to orthogonal factors, since we have nowhere supposed the levels to be linearly related. But we can also rotate the centroid factors without immediate relation to the structure in the sample or to direct propositions

for it. *A dependency analysis can be undertaken with psychoanalytic or other principles to guide the rotations.* That is, there can be an expectancy that psychoanalytic dynamisms will be operating for the case and that these will be discernible as factors. By "dynamisms" we mean such processes as idealizations, rationalizations, fixations, and the like defense mechanisms. These factors would in no way contradict any required for conformity with the variance design.

Possibilities of this kind are of considerable practical importance. It means that there can be alternative, but equally acceptable, solutions for the centroid analysis.

Moreover, there are still other, more abstract possibilities open to us. Higher-order explanations of dynamic matters may be at issue. We might find that factors $k_1, k_2, k_3, \ldots$, for Dora, for example, are in such a relationship that a change in one can allow us to predict what must happen to others, and so on. All such analytical possibilities are open to us. The correlation of Dora's self-arrays (Q-sorts) and their factor analysis are perhaps the most interesting matters, methodologically, that we touch upon, for they may reach beyond the confines of the independencies postulated at the outset.

We shall proceed to a concrete study of the kind under consideration for Dora. Meanwhile, it is important, methodologically, to notice that much more can be involved in a structured sample than the structure itself would suggest. The operations of a person, such as Dora, can give rise to innumerable nuances and facets of behavior; it is the virtue of a good theory to show one *what* to look for under various conditions.

Enough has been described to give at least a first glimpse into these possibilities of studying the subjectivity of a person. In Dora's case her self-descriptions are used to probe into her behavior, *retrospectively* regarded, such as is encompassed in psychoanalysis by *free association.* If there is any lawfulness about, the methods of retrospection and of free association must surely point in the same direction. The methodology, however, can be applied to any subjective operations of a person, for *projections* as much as for retrospections. There is a lovely piece of poetry by Corwin which reminds us that the stars and worms have been taught to live together; here, too, it seems to us that improbables have been brought together in a new way, opening the door to experimentation of unlimited scope.

FACTOR STUDY FOR A CASE: ROGERG

We do not have a Dora at our command, nor is it our province to analyze a patient and to theorize about her or him along psychoanalytic or self-theoretical lines. But it is possible to illustrate the technique outlined for Dora's case.

With an assistant (Mr. J. C. Nunnally) we put together a sample of 60 self-referent statements made by an experimental subject whom we shall call "Rogerg." The sample was unstructured and was of the following nature:

1. I sometimes feel as though I should run out of the room I am in, scream, or burst into tears.
2. I am strongly critical and skeptical, but I keep these feelings to myself.
3. Although it is usually not apparent to others, I am really quite hypocritical, in many ways.

The young man, aged twenty-one years, had been mentally a little sick, as statement 1 suggests. We had interviewed him, had his confidence, and knew a great deal about his difficulties over the previous several years. At intervals of about a week apart Rogerg gave a self-description under specific conditions of instruction, which were such, it was thought, as might impinge upon crucial situations in his present and past sickness. In all he gave fifteen such self-descriptions, according to the scheme in Table 2.

All fifteen appraisals were made by Rogerg himself, all from his personal frame of references—all are his own conceptions. The variates were introduced to cover significant phases or impacts in Rogerg's life. Great difficulties in adjustment had been manifest in the period from nine to sixteen years; during this time and earlier, his mother had encouraged him to be an artist, and this was still a source of conflict. He was involved in discussions of a general kind with Dr. Stephenson (variate No. 14), in whom he placed considerable trust. He had always had many dealings with his neighborly uncle and aunt (No. 13), with whom he had recently gone to live. Each variate has some purpose or point of this order.

The fifteen variates were correlated and factored (centroid), with the results shown in Table 3. Three factors are adequate for the data given, unrotated and orthogonal, as I, II, and III.

We next rotate the factors in any *dependent* manner that may occur. Consider, first, a rotation which provides the loadings given

in Table 4. The procedure here was first to plot I and II and to rotate through No. 14, so placing many of the variates (Nos. 1, 2, 4, 5, 7, 9, 14, and 15) on a near-zero value for factor II′, but as high as possible for I′. The new I′ was next plotted against III, but the plots are now scattered pretty widely over the four quad-

TABLE 2

Statement No.	Designation	Variate
1 and 2..	*Self* (July, October)	Rogerg describes himself as he judges himself to be at this time (July, and again in October).
3.......	*Ideal* self	A self-description of what he would like to be, or to have been, *ideally*.
4.......	*Self* (9–16 years)	His self, as he now remembers it, when he was between nine and sixteen years of age. (He was very unhappy at school, and there were reasons to suppose that this period was especially significant in his life-history.)
5.......	*Self* (16–18 years)	His self, as he now remembers it, when he was between sixteen and eighteen years of age. (Again important, almost traumatic, events occurred at about eighteen years of age.)
6.......	*Should* self	His self as he thinks it *should* be now, having regard to everything.
7.......	*Becoming* self	His self as he thinks it is gradually becoming, i.e., its direction now.
8.......	*Friends* self	How he believes his friends regard him. (Rogerg conducted himself in a Bohemian fashion with certain friends, on occasion.)
9.......	*Mother saw*	This is how, he believed, his mother thought of him, that is, her conception of him as he was.
10......	*Father saw*	Similarly with respect to father—this is the kind of person Rogerg believed his father thought he was. (We need scarcely say that the father-mother situation had been particularly tense.)
11......	*Mother wanted*	This is the kind of self that Rogerg believed his mother wanted him to be—not as he was, but her ideal for him.
12......	*Father wanted*	Similarly with respect to father—this is what he believed his father wanted him to be like.
13......	*Relative role*	An uncle and aunt played a special part in Rogerg's life, and he was asked here to describe the kind of self he adopted with respect to these relatives, i.e., this is how he appeared to these relatives—again, of course, from his self-referent point of vew.
14......	*Stephenson* self	We had interviewed Rogerg on many occasions and were regarded as a kind of mentor, a helper. Rogerg here described what he believed we thought he was like.
15......	*Art* self	One of Rogerg's stresses was centered upon a training in art about which his mother had been especially formative. He here described himself as he saw himself in his art milieu—this is how he thought of himself in this setting.

TABLE 3

CORRELATIONS FOR ROGERG'S SELF-NOTIONS; $n = 60$ STATEMENTS

		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	Centroid Factors (Unrotated) I	II	III
1	Self (July)	—															81	22	28
2	Self (October)	78	—														83	15	14
3	Ideal self	−15	−28	—													−42	29	51
4	Self (9–16 years)	50	56	−53	—												70	12	−31
5	Self (16–18 years)	70	82	−31	66	—											87	11	−09
6	Should self	53	35	24	08	18	—										36	13	64
7	Becoming self	48	61	−22	43	74	19	—									70	11	−08
8	Friends see	−02	−02	05	−21	00	17	06	—								06	−52	33
9	Mother saw	40	38	−41	61	54	−04	41	−16	—							61	11	−49
10	Father saw	15	05	06	14	17	25	13	22	10	—						20	−11	18
11	Mother wanted	−26	−26	47	−23	−37	05	−34	−34	−33	−06	—					−46	52	24
12	Father wanted	−40	−53	29	−29	−41	−24	−16	−15	−12	−03	26	—				−49	24	−20
13	Relatives self	55	46	09	22	37	54	39	00	05	10	11	−18	—			47	40	44
14	Dr. Stephenson sees	76	81	−40	66	81	32	70	−11	58	15	−33	−44	48	—		91	21	−05
15	Art self	42	41	−47	60	52	05	35	−23	73	−06	−32	−11	25	61	—	62	23	−46
	Percentage variance																62	12	19

rants, and we choose to rotate through No. 3, Rogerg's *ideal* self. We did this because, in practice, there is some evidence that the ideal self can be a nexus about which much in the personality is shaped. The new I″ and III′, along with II′, are the factors listed above. As they stand, we may proceed as follows:

a) Factor I″ is a widely spread concept he has of himself, as he is, has been, and will be; he believes that his relatives and Dr. Stephenson see him like this. It is not in any way like his *ideal*, which has zero loading on this factor.

b) Factor II′ is centered on his *family* (as his father and mother *wanted* him to be). His *friends*, he thought, had the opposite idea about him in this respect (No. 8). His *ideal* has 0.37 loading in it.

c) Factor III′ is centered about his *ideal* (0.61). It picks up something in No. 6 (*Should*), No. 8 (*Friends*), and as his mother *wanted* him to be (No. 11). Diametrically opposed to it are Nos. 9 (as his *mother saw* him), 15 (Art self), 14 (Stephenson), and the *becoming* and existing selves (Nos. 1, 2, 4, 5, 7).

TABLE 4

ROTATED FACTORS

			I″	II′	III′
1	Self (July)	1	87	04	−24
2	Self (October)	2	79	−03	−36
3	Ideal	3	00	37	61
4	Self (9–16)	4	43	−03	−66
5	Self (16–18)	5	64	−08	−56
6	Should self	6	66	05	33
7	Becoming self	7	55	−05	−46
8	Friends self	8	13	−52	31
9	Mother saw self	9	25	−03	−76
10	Father saw self	10	24	−15	05
11	Mother wanted	11	−13	61	38
12	Father wanted	12	−46	36	07
13	Relatives role	13	71	29	05
14	Stephenson saw	14	74	00	−56
15	Art self	15	30	09	−75

One might argue that I″ is Rogerg's "general self" or his genetic self; that II′ is his *family* self; and III′ his *ideal*.

But we can look at the respective factor-arrays in detail. These are given in Tables 5, 6, and 7 for the top ten statements or so, and the lowest ten or so statements in each factor-array.

Factor I″ has its highest loadings on variates 1, 2, 13, and 14, all of which concern Rogerg's *present* milieu. But the loadings reach into his adolescence (Nos. 4 and 5) as well. In a general way it is

difficult to convey to the reader the appositeness of the factor-array in this broad genetic respect. Rogerg is, and usually has been, quiet, mouselike in timidity, pacifistic. To hate or even resent is and has been always beyond him over these years. But he is aware, apparently, of crudities, insults, false ideals, and pettinesses around him; as a result he is critical and skeptical, but not overtly. There is a harsh

TABLE 5

FACTOR I″: LISTS OF THE BEGINNING AND END OF THE FACTOR-ARRAY FOR I″

Highest in I″		Lowest in I″	
Score	Statement	Score	Statement
10	I have great faith in many of my own ideas.	0	I can really hate strongly at times.
10	I am seldom worried about things in general. Easy going.	0	I easily become resentful of others. I greatly dislike being taken advantage of.
9	I am often strongly critical and skeptical, but I keep these feelings to myself.	1	I am indifferent to social injustice such as racial discrimination.
9	I seldom overtly react against the tastes of others even if they appear crude to me.	1	I make a conscious effort to conform to adult standards.
9	I think of myself as being compliant and obliging—ready to do whatever I am asked to do.	1	My feelings are easily hurt by the slightest remarks of others.
8	I enjoy working in co-operative efforts.	2	I am little interested in what is going on in the world, such as social welfare and the war situation.
8	I ignore insults rather than attack them.	2	I feel pleasantly exhilarated when all eyes are on me.
8	I often feel that the people around me are acting according to false ideals.	2	I often become entirely absorbed in thinking about myself.
8	I am indifferent to petty things. Many things are not worth bothering about.	2	I am usually aloof, but respond to polite addresses.

and unsympathetic milieu about him which he meets with compliance, obedience, and fantasy (the reference to "working in co-operative efforts" relates entirely to a wish to emigrate to Israel, there to work in a land of milk, honey, and ideal community life). Naturally his mother did not *want* such a personality for him; and the "*father wanted*" variate is negatively loaded with the factor. The father, indeed, was assertive, a man-of-the-world, and no doubt

would have liked his son to be a chip off the same block. However, we shall look at the factor again later, when we perform the second of our rotations. Meanwhile, factor II′ is how Rogerg's mother *wanted him* to be (again, of course, according to Rogerg)—controlled, hardworking, sensible, respected. The factor is fascinating, because Rogerg provides a negative loading in it for the way his friends see

TABLE 6

FACTOR II′: LISTS OF THE BEGINNING AND END OF THE FACTOR-ARRAY FOR II′

Highest in II′		Lowest in II′	
Score	Statement	Score	Statement
10	My ideas are usually sound and sensible. I don't let my whims and fantasies enter into my practical decisions.	0	Enjoying the present is more important to me than working for things in the future.
10	I am deliberate in most of my actions and cautious in making decisions.	0	I am careless in my personal habits and take poor care of possessions.
9	I rarely become very excited or thrilled.	1	When at a boring party I might become drunk to escape the monotony.
9	I like going to school. After a vacation I realize that school can be quite pleasant.	1	I guess I would be called soft-hearted. Tears come to my eyes rather easily.
9	When working at something, I don't rest until a satisfactory result is obtained.	1	I have often thought of myself as being neglected and unloved.
8	I try to be self-controlled. When I am bored or annoyed, I try not to show it.	2	I am more concerned about large social issues than petty, everyday personal problems.
8	I make a conscious effort to conform to adult standards.	2	I often become entirely absorbed in thinking about myself.
8	I like to be seen and be heard. It makes me feel good to know that I am impressing others.	2	I think of myself as being compliant and obliging—ready to do whatever I am asked to do.

him; and, in point of fact, among a few friends he has several times displayed the negative counterpart in detail—he gets drunk, cries sentimentally, feels the utter neglect of a depressive, is careless, and "enjoys" the passing moment in a carefree Bohemian fashion. Variate No. 8 is an accurate account, indeed, of the self with which he confronts his friends on these abandoned occasions. The factor thus represents a mother-identification in its positive form, and a complete rejection of it in its negative. One is just as true as the

other in the appropriate interactional setting. It must point to at least one of Rogerg's problems, and he may, or may not, be aware of the connection between his mother's wishes for him and their rejection before his permissive and sympathetic friends.

Factor III′ has to do with Rogerg's expressed *ideal*, his ideal self. And, knowing him, it could scarcely express his conscious desires in

TABLE 7

FACTOR III′: LISTS OF THE BEGINNING AND END OF THE FACTOR-ARRAY FOR III′

Highest in III′		Lowest in III′	
Score	Statement	Score	Statement
10	I make a conscious effort to conform to adult standards.	0	I have great faith in many of my own ideas.
10	I feel pleasantly exhilarated when all eyes are on me.	0	I often feel that people around me are acting according to false ideals.
9	I like going to school. After a vacation, I realize that school can be quite pleasant.	1	I have fallen into the habit of becoming indifferent to what goes on about me.
9	I am indifferent to social injustice, such as racial discrimination.	1	I often forget what I am doing and lose myself gazing at the trees and the sky.
9	I could always play freely with other children.	1	I would like older people to be more idealistic. I hate to see them engage in unbecoming pettiness.
8	I had rather participate in games and conversation than watch others.	2	I sometimes feel as though I should run out of the room I am in, scream, or burst into tears.
8	I like to be seen and heard. It makes me feel good to know that I am impressing others.	2	I am often strongly critical and skeptical, but I keep these feelings to myself.
8	I can really hate strongly at times.	2	I am indifferent to petty things. Many things are not worth bothering about.

this respect in a better way or be a better contrast for the condition represented by factor I″. It expresses a desire for freedom, a wish for adulthood, for self-display which is enjoyed, and for freedom to play happily (which has never been possible for him). Freedom to hate, if need be. There is disillusionment, too, as witnessed by the utter deflation of the "faith in his own ideas" and in the "false ideals" of others. Even social discrimination and concern about social injustices

are thrown to the wind, as though it would indeed be pleasant, for once, to be free from their oppressive connections.

Such, then, is the good sense made by these factors, so rotated. We hope that the R-technique factorist is not alarmed by our "interpretations"; he can always try a case for himself. Meanwhile, we believe that matters are very considerably as we have interpreted them and that they are there to be seen and heard, as grass is green and the song of a thrush lovely to hear. Factor I″ indicates a reaction-formation in some way. Factor II′ is one of Rogerg's problems,

TABLE 8

ROTATED FACTORS

		Variate	I^a	II′	III^a
1	Self (July)	1	50	04	74
2	Self (October)	2	59	−03	64
3	Ideal	3	−58	37	19
4	Self (9–16)	4	76	−03	19
5	Self (16–18)	5	74	−08	47
6	Should self	6	−10	05	73
7	Becoming	7	61	−05	37
8	Friends self	8	−25	−52	22
9	Mother saw self	9	80	−03	00
10	Father saw self	10	02	−15	25
11	Mother wanted	11	−40	61	−01
12	Father wanted	12	−21	36	42
13	Relatives wanted	13	17	29	68
14	Stephenson saw	14	76	00	52
15	Art self	15	80	09	04

concerning mother-identification. Factor III′ represents his yearning, an awareness of what is *possible.*

But we now have a different solution to consider. In this we retain factor II′ as it stands, but we rotate I″ and III′ into new positions, providing factors which we call I^a and III^a, respectively, by placing an axis through variate No. 9, instead of through No. 3 (as was the case for I″ and III′). The new factors, along with II′, are shown in Table 8.

The factors are again orthogonal. Factor I^a centers upon Rogerg's conceptions of how his mother saw him, what he was like in adolescence (nine to sixteen) and at sixteen to eighteen years of age and in his art milieu—all of which were laid down, so to speak, in adoles-

cence. It is interesting that Dr. Stephenson is supposed to conceive of him in this way too—another mother-figure (variates Nos. 4, 5, 9, 14, and 15 carry the high loadings). The factor is opposed to his *ideal* (loading −0.58) and "mother *wanted*" (loading −0.40). His history indeed shows that his art career was his mother's choice rather than Rogerg's own. The factor, therefore, strongly suggests

TABLE 9

FACTOR I^a: LISTS OF THE BEGINNING AND END OF THE FACTOR-ARRAY I^a

Highest in I^a		Lowest in I^a	
Score	Statement	Score	Statement
10	I have great faith in many of my own ideas.	0	I make a conscious effort to conform to adult standards.
10	I often feel that the people around me are acting according to false ideals.	0	I am indifferent to social injustice, such as racial discrimination.
9	I am indifferent to petty things. Many things are not worth bothering about.	1	I like going to school. After a vacation I realize that school can be quite pleasant.
9	I have fallen into the habit of becoming indifferent to what goes on about me.	1	I easily become resentful of others. I greatly dislike being taken advantage of.
9	I am often strongly critical and skeptical, but I keep these feelings to myself.	1	My feelings are easily hurt by the slighting remarks of others.
8	I would like older people to be more idealistic. I hate to see them engage in unbecoming pettiness.	2	I feel pleasantly exhilarated when all eyes are on me.
8	I often forget what I am doing and lose myself gazing at the trees and the sky.	2	I like to be seen and be heard. It makes me feel good to know that I am impressing others.
8	I sometimes feel as though I should run out of the room I am in, scream, or burst into tears.	2	I could always play freely with other children.
8	For me, lovemaking is a serious matter.	2	I can really hate strongly at times.

that his *adolescent* self is at issue—this is how he confronted the social milieu a few years ago. Factor II′, as we saw, remains untouched, and is to be regarded as Rogerg's problem. Factor III^a is centered on Rogerg's *should be* self, on his *present* self (Nos. 1, 2), and upon how his relatives perceive him now (No. 13)—all, of course, from Rogerg's own frame of reference. The factor-arrays for I^a and III^a can be calculated in the usual way, providing the data in Tables 9 and 10.

Instead of interpreting factors I^a and III^a *de novo*, it is better to compare results for the two sets of rotated factors. As we see, II' remains the same, but I'' and III' are differently divided to provide I^a and III^a.

In many ways I^a and I'' are still alike. The same statements, for the main part, appear in the two extremes of the respective factor-

TABLE 10

FACTOR III^a: LISTS OF THE BEGINNING AND END OF FACTOR-ARRAY III^a

Highest in III^a		Lowest in III^a	
Score	Statement	Score	Statement
10	I have great faith in many of my own ideas.	0	I can really hate strongly at times.
10	I enjoy working in co-operative efforts.	0	I am indifferent to social injustice, such as racial discrimination.
9	I seldom overtly react against the tastes of others, even if they appear crude to me.	1	I easily become resentful of others. I greatly dislike being taken advantage of.
9	I am seldom worried about things in general. I guess you would say that I am easygoing.	1	I make a conscious effort to conform to adult standards.
9	I am often strongly critical and skeptical, but I keep these feelings to myself.	1	I am little interested in what is going on in the world, such as social welfare and the war situation.
8	I think of myself as being compliant and obliging—ready to do whatever I am asked to do.	2	My feelings are easily hurt by the slighting remarks of others.
8	Although it is usually not apparent to others, I am really quite hypocritical in many ways.	2	I feel pleasantly exhilarated when all eyes are on me.
8	I seldom think of people in terms of male and female. If I am interested in someone, it doesn't matter whether it's a man or woman.	2	I am usually aloof but respond to polite addresses.
8	I ignore insults rather than attack them.		

arrays. But there are also notable differences. Factor I^a has negative loadings on Rogerg's *ideal* (No. 3), whereas I'' had zero. Rogerg's *should* self occurs in I'', but not I^a; and the role around his uncle and aunt is very differently indicated—it appears in I^a and not in I''. It seems that I^a is distinctly Rogerg's adolescent self in retrospect. He was extremely unhappy at his school then, and we know that he had indeed run out of classes, screaming and in tears. Idealized love had

a place at that time, and he was often lost in daydreams. He could not play with others and seemed utterly incapable of any adult role, or contemplation of it. We know that he reacted with considerable intellectualization to these adolescent stresses, and this is well brought out by the very first statement in I^a—"great faith in his own ideas." So intellectualized was he, indeed, that he was given a scholarship to the University of Chicago at eighteen years of age. The emphasis in I″, on the other hand, is toward a more easygoing attitude, which leaves him aware of crudities, pettinesses, and the like, but about which he can only be inactive, except for the fantasy about "co-operativeness," i.e., in Israel. Factor I″ was a mixture, indeed, of his adolescent and *present* self, and I^a is more purely his adolescent self.

The differences are also marked for III′ and III^a. Factor III′ represented Rogerg's *ideal*, whereas III^a is centered instead upon how he thinks he *should* be. Remember that III′ expressed a desire for freedom, a yearning. Here it corresponds, instead, to Rogerg's reaction to an unsympathetic world—to co-operate (in fantasy) in an idealized society, to live by ideas, to be pacifistic, and yet to realize that it is all hypocritical and a façade. Reaction-formation and intellectualization are again evident—picked up from I″. It is indeed his *present* self *in excelsis*, as shown by the loadings at 1, 2, 6, and 13.

Thus the first rotation brought his *ideal* self forward, against a self (I″) which was partly *present* self and partly *adolescent* self. The second separates the latter but loses the *ideal* self in the process. Meanwhile, the mother-identification factor remains untouched.

We illustrate in this way, then, several fascinating matters, methodologically regarded. *First*, the empirical subdivision of a personality into its different selves is clearly indicated—it is surely from this basis that the proper study of personality should begin, as the present-day behavioral self-psychologists have averred. But where they infer and aver, we provide direct operational definitions. *Second*, the different rotations bring different facts to light, no one more true than another. It is indeed true that Rogerg has yearnings for freedom (III′), and no less certain than his *present* self (III^a) and his adolescent self (I^a) are ever with him, unresolved. No less certain is the identification and rejection in factor II′—mother's ideal and the

Bohemian outbursts are admirably counterpoised! But it is a separate matter to determine whether Rogerg has insight into these different, but interrelated, aspects of him-*self*.

We confess that the above demonstration pleases us, methodologically and psychologically. It will be the forerunner, of course, of much else and many studies of the kind. Here indeed we solve an age-long search for the self—scientifically studied.

The personality theorist is now at liberty to ask his questions. If he believes in the unity of the self, he will no doubt express concern at our subdivisions into orthogonal parts. If he likes security, he will be alarmed at our twisting axes. Mere verbal stereotypes, we believe, are all that there is to fear. The facts are as we find them. No doubt, in the present example, Rogerg maintains a certain unity by way of reaction-formation and intellectualization—he comes to terms with life in this manner. But his personality is really split wide open. He has at least three selves, it seems, all relatively inflexible. These are some ways in which he confronts his milieu. But he also knows what he might achieve, or so it seems; and if he could come to grips with a self which has the properties of his "ideal self," which would be flexible enough to incorporate all his current experiences realistically, a kind of harmony might indeed replace the present disunity. The selves with which he confronts the world at present obviously intrude into his behavior. There should be one, instead, perhaps, which is *based* upon it, as free *transaction*.

However, we must leave these matters for the present to others. Meanwhile, two consequences are worth noting. Clearly, we could continue, perhaps for a long time, before the 60 statements pall or become irrelevant, to add to our (15 × 15) table of correlations. Spring comes, and Rogerg perhaps takes an important decision for himself—mother is dead, he can now give up his art training and concentrate instead upon his college work. What will happen to his *self*, the concept of himself, now? Will his *ideal* alter? The whole science of personality consists of being able to predict the answers of such questions, in factor terms. Or, if we place Rogerg in experimental situations, designed to give insights into himself, as in psychoanalytic or nondirective therapy situations, can we predict the changes? The methodology now exists for such studies, as never before.

In general, we should say, perhaps, that we might hope to compile a rather different set of self-notion statements from time to time if we pursue the study of a single case such as Rogerg. Under psychoanalysis, for example, notions will arise that would never do so normally. In such an event the new "sample" could be devised, and the process of analyzing it begun all over again—the time spent is not exorbitant, having regard to the fact that a single case is being studied.

DISCUSSION

The factors arrived at in the above studies represent, we have supposed, some more or less usual ways in which a person operates, whether he is aware of it or not. They are comparable to those discussed by Cicero, as noted at *a*, page 243, above, and, indeed, are merely an operationally defined elaboration of the common-sense view that a person behaves in such a way, or has done so in the past, and may do so again. Our factors, however, are orthogonal affairs, and the critics will wonder whether life can be quite as geometrical as all that. We would again give the reminder, however, that factors such as those discussed for Rogerg are likely to be in dynamic relation to one another—a matter that can be put to proof. If one factor alters, owing to therapy or to other causes, then it is likely that others will be modified *pari passu*. Some there may be which are almost immutable for the person, but most, we believe, are likely to vary with the interactional setting. Indeed, it was the whole purpose of the study by Edelson and Jones (62) to consider these possibilities. The factors are to be regarded as approximations to lived behaviors: that they are orthogonal is neither here nor there—the orthogonality is a matter of convenience only, to give power and criticalness to our thinking about the subject. The studies referred to above were for single persons, in terms of their own language: but single persons can be studied, almost appositely, in terms of statements which represent a particular interactional setting, presumed to be much the same for all persons (62).

Meanwhile, an example has been provided of a study on a single individual, in terms of highly particular statements and variates that could mean little or nothing to anyone but Rogerg. The science is provided by the Q operations and by the theoretical framework

concerning intellectualization, reaction-formation, identification, and similar, mainly psychoanalytic, concepts, although self-theorists may have other ways of interpreting the facts. The cards can now all be laid on the table, however, to see what really goes with what from the standpoint of the internal framework.

Nor can we refrain from making brief reference to R-technique again. Our critics have been saying for a long time that there is nothing really new in Q-technique. This can be only because they have never really read our early papers or did not take them seriously enough. It is plain for all to see that R-technique could never even begin to study a single case and that not a single postulation occurs anywhere in the above study about individual differences. The R-factorist, indeed, would set about some of the matters we have had under discussion in quite a different way and could never reach the selves we have brought to operational terms. They might seek to study intellectualization in *general*, or degree of mother-identification, or idealization, all as general principles, quantifiable in terms of univariate instruments and individual differences. No doubt some useful information might result along such lines, on a par with the kind of information that tells us how many men are bald, how many women smoke, and what the density of illiteracy is in South America. We should also refer to a detail in relation to the R-approach to personality: our Rogerg would be rated very low by anyone for the activity factor of Studman and Thurstone (191). But we now see that this might be merely a reflection of the way Rogerg has to confront his unsympathetic milieu (factor I″ or III^a): he has to retreat from it by being inactive. An alternative is that Rogerg has a low endowment of the R-technique temperamental *a*-factor and that this essentially accounts for his inactivity. Both possibilities exist: Rogerg may *have* to be inactive, for the dynamic reasons given, and this may at the same time comport with his natural endowment. We doubt whether anyone could give much credance to *a*-factor as a causative agency in itself: the dynamics of the case suggest otherwise. The conclusion could well be, indeed, that the supposed or real capacities of a person may fit where they can in the personality but are not essential arbiters or even conditions of it.

A THEORY OF ATTAINABLE SELVES

It should be asked, What kind of theory should the self-psychologist seek to foster, having regard to the discussion up to this juncture in this chapter? We can say something about what it should *not* be, what it does not involve, and what lines it perhaps can best take.

It seems clear that self-psychology can have a powerful ally in Q-methodology. But this could lead to a mad scramble of fact-finding, undirected by any important theoretical considerations; and, clearly, what is essential at the present stage of work on the *self* is a good theory about the matter. By this we mean one that gives rise to pregnant propositions, that is, to interesting discoveries about *subjectivity*.

History scarcely provides such a theory. William James proposed his theory of the Pure Ego, hierarchically organized: the person is first aware of his physical body self, his social self is superimposed upon this, and still later his spiritual self develops, giving unity to life's purposes; ultimately a pure ego is grasped. This, it seems to us, is clearly a construction with empty boxes. An R-factor analyst, such as Burt (41), might recommend to us a "four-factor theory of the self." It is usual to distinguish four types of factors—*general*, *common*, *specific*, and *error*; therefore, it might be argued, selves discovered along Q-technique lines could be of four kinds, corresponding to these classes of factors. We might expect X to carry about with him a very general conception of himself, which he employs widely in interactions; or he may have some common factors, analogous to the "conceptual roles" of Snygg and Combs (147), of narrower applicability. Still others might be *specific* to a particular event, as when X is half-drunk and has grandiose self-conceptions. Finally, there might be purely *accidental* selves, never repeatable, uniquely occurring, as when X plunges to death in an air accident. But this is scarcely a theory in a scientific sense —it is merely a logical compendium. Lewin's studies (104), Sherif and Cantril's (143), and much of Koffka's earlier theorizing (97) concern ego constructs which are analogies taken from topology, physics, or dynamics. But no self-operations are ever at issue from which the constructs necessarily follow.

Consider, however, the psychoanalytic concepts of Id, Super-Ego, Ego, and Ego-ideal. The greater part of each of these concerns unconscious matters. *This means that X can come to no self-referent notions about much of his behavior.* The psychoanalyst, of course, concerns himself with repressive and reactive influences which make it impossible for X to have self-notions that he could otherwise have, and it is an important objective in psychoanalysis to make such self-referent notions possible, to bring them into daylight as insightful musings and reflections, freed from repression, guilt, and anxiety conditions which would ordinarily be in attendance upon them. Each person, then, may be considered as capable of many selves, some of which he can be aware of, but many of which he cannot recognize. All are attainable, however, in the sense that what was repressed or just overlooked or what was *reacted* to because of inhibitory influences can be brought to light. Thus, in addition to the *self-descriptions* provided by cases such as Dora, their *factors* are sometimes, in effect, self-descriptions that they *could* have given, in addition to those that they were called upon to describe. It will be at once apparent that these might be of interest to a psychoanalyst, as well as to any psychologist. It might be supposed, then, that self-psychology could make a beginning by determining what are X's attainable selves, whether X has insight into all of them, and whether evidence for them can be reached very quickly along Q-method lines, even when X himself is unaware of them. With some such theoretical standpoint, with a pragmatic leaning, it would seem reasonable to expect interesting discoveries.

Rogers and his school, and Mead before them, have gone further. We would refer the reader, for an answer to our initial question about what theory one should pursue, to the recent studies of Rogers (129) on the case of "Mrs. Oak," and of Nunnally (120) on the case of "Miss Sun." We cannot take the reader into these clinical studies here. The concern, however, from our methodological standpoint, is with many general theoretic propositions, of the kind asserted originally in Rogerian self-theory as postulates. These are now brought to operational terms in a direct manner by way of self-assessments, under the many different and varied conditions of instruction that our study of Rogerg illustrates. This, indeed, is the prototype of much to come.

CONCLUSION

"The beginning of an acquaintance with persons or things," George Eliot wrote, "is to get an outline for our ignorance." This chapter may be said to have such an outline as its object.

Our contribution will be misunderstood, however, if it is thought that we are advocating a "means-centered" approach to the study of the self. At its best it should be impossible to separate methodological, statistical, and psychological matters in experimental work. Q-technique is not a statistical device that one applies to data, as a bandage is applied to a wound, for want of a better specific. On the contrary, it entails the dovetailing of method, theory, and experiment. The study of Rogerg, it seems to us, holds in it, methodologically, such a happy coalescence. The methods now exist, therefore, for putting subjectivity upon a scientific footing. Moreover, the great analytical advantage of factor analysis is available, which permits the investigator to transcend his postulations and to make inductive explorations and higher-order, more abstract explanations of effects. It is along such lines, we suspect, that a true *theoretical* psychology might be reached.

However, perhaps our contribution will be as little understood as our earliest were on Q-technique, and we shall be accused (indeed, this has already occurred [50]) of a Machiavellian desire to restore the soul to psychology, at a pre-Humian level. It is for this reason, among others, that we have been careful to assert that our concern is with behavior, as soundly and as concretely as any that has been studied for rats, pigeons, cats, dogs, and hens. We reject phenomenology for the same fundamental reasons.

A person's self-reflective statements and musings are, in principle, unique and irreversible events; his self-assessments are of the same nature, and their factors (which one might call K) may likewise be wholly idiosyncratic and particular to a case X. But we would scotch the canard that, because of this uniqueness, these can be of no concern to science. On the contrary, it has its beginnings here; for our concern, of course, is not to catalogue unique factors but to discover their laws. If we can show that factors $K_1, K_2, K_3, \ldots, K_m$ for a person are in dynamic relationship, such that if one is changed the others alter *pari passu*, science is in the making. It would be lawful if, given the factors for X as he is *now*, we can predict success-

fully what his factors will be in a specified future, or if we can predict the factor composition of any of his subsequent self-notions. But we have also seen, as in the case of Rogerg and as mentioned for Dora, that factor analysis can be undertaken from a *dependent* standpoint, that is, with psychoanalytic, self-theory, or other theoretical standpoints to govern the analysis.

Thus, far from these unique *K*-factors being unscientific, a region of ghosts and souls, they could become central to our whole science. Along the above lines, we are sure, Ward's search (197) for a schema within which to study the concrete person from his own *synthetic*, subjective, standpoint would have ended.

CHAPTER XII

Q-METHODOLOGY AND PERSONALITY

Q-TECHNIQUE AND THEORIES OF PERSONALITY

PSYCHOLOGICAL literature is replete with different theories of personality, as the bare names of Freud, Adler, Jung, Spranger, Klages, Kretschmer, Shand, McDougall, Lewin, Allport, Murray, Mead, Rorschach, Sherif, Cantril, Szondi, Rogers, and many others testify. It is certain that Q-technique can permit of ready experimentation with the doctrines of almost any of these psychologists and their plethora of theory. Its applications reach into self-psychology, personality types, the processes of personality (such as identification, rationalization, and the like), and the formal study of personality from psychological and sociopsychological standpoints.

It seems, however, that no one is sure about what to encompass by the term "personality." There are so many meanings for it that it appears almost useless for scientific purposes. Murray (117), Cattell (47), Kantor (88), Kretschmer (101), Burt (40), and others, from very different standpoints, regard personality as a rubric for everything that can be found out about a person—his physique, abilities, skills, traits, attitudes, tastes, opinions, knowledge, and all else. For our own part we propose to mean, instead, the possibility of a distinctive "character" for a person, comparable methodologically to Cicero's category *d* (p. 243): something that raises the person above his fellows is at issue. It is central to the subject himself—*his* own standpoint is somehow involved. What this means will become clearer as we proceed.

ELEMENTARY DEFINITIONS

A few elementary matters of definition require mention. First, about personality *traits*. Man, we believe, enters into an infinite number of behavioral segments day after day. He saves someone's

life—the *deed* we call "brave," but we are also likely to imply that he is a *brave* man. That is we impute *bravery* to him as a causal agency. But when he saved the life he perhaps felt no noble thoughts —on the contrary, he may well have cursed the foolish person who placed himself needlessly in the situation of danger. Traits, then, (1) may name a whole behavioral segment as it is observed from the exterior, or (2) may be the name for an inner attribute, imputed to someone as a causal agency, or (3) may be self-descriptions and concomitants of behavior.

The first type, as we see for *bravery*, need have no correspondence between what is imputed from the observer's standpoint and what is, in fact, experienced by the person. The third comprises the self-referent statements of earlier chapters. The second holds in it the qualities we impute to our neighbors, from an external framework. Psychologists have dynamic theories here at issue in this way, such as when they suppose that X is *orally aggressive, anal compulsive*, or the like. Common-sense regard might say, instead, that X is greedy or an evil-minded person.

It is usual to distinguish between traits of *character or conduct* and traits of *temperament*. In the latter there is a certain direct correspondence between "inner" and "outer" regard of the behavior; in the former not necessarily so. A *cheerful* person looks it and also feels it inwardly. A man who has saved a life does not necessarily feel *noble* as he does so.

The systematic study of personality, in the past, has usually been from the external standpoint of character, conduct, and temperament rather than from the internal one of self-referent traits. The latter were regarded as unreliable and subjective, and everything was done to discredit their study; instead, a false objectivity was sought after, in which *deeds* and supposed causative *traits* were often confounded at the outset. The results of studying traits from the external frame of reference only, as Cattell (47) has done, seems to be barren of any theoretical standpoint and is often seriously mistaken as to fact. We have shown elsewhere how one of Cattell's tables of correlations, in R-technique, should have been analyzed from the internal standpoint *of the persons who made the assessments*, instead of, as it had hitherto been, from the standpoint of the persons about

whom the assessments were externally made. Our conclusion was that semantics is at issue in such studies rather than any psychology.

However, the study of *traits*, as socially qualified descriptions of behavior, is worth pursuing along Q-lines.

Q-TECHNIQUE STUDIES AND TRAITS

Cattell (47) is well known for his studies of so-called "general traits" of the following kind:

1. Obstructive—obliging
2. Changeable—constant
3. Attention-getting—self-effacing.

These traits were originally described in brief paragraphs: thus, *cheerful* was defined by Cattell as follows:

> Generally bubbling over with good cheer. Optimistic. Enthusiastic. Prone to cheerful, witty remarks. "Laughterful."

Its bipolar counterpart was as follows:

> *Depressed, solemn:* earnest and solemn most of the time. Not easily moved to laughter. Seeming slow and depressed rather frequently.

For Q-purposes we require many brief statements and a basis for constructing any number of samples of a kind. From each of the 70 paragraphs of Cattell's list for 35 bipolar traits we took three short phrases, to make three parallel sets of 70 each. Thus *cheerful* was separated into (1) cheerful, (2) enthusiastic, (3) laughterful; and *depressed* into (4) earnest, (5) solemn, (6) depressed rather frequently. For ease of application, three sets, *A*, *B*, and *C*, of 70 each were taken, each containing an item from each of the equivalent triads, such as 1, 2, 3, or 4, 5, 6. Set *A* contained 1 and 4; set *B*, 3 and 5; and set *C*, 2 and 6; and so forth. Every generalized trait, therefore, was represented in each of the sets *A*, *B*, and *C*. These sets can be regarded as only roughly "parallel"—a word like "enthusiastic" scarcely means the same as "cheerful." No set contained direct antonyms or mere negatives: the terms "active" and "inactive," for example, did not occur in the same set or sample of 70 items. The three sets *A*, *B*, and *C* constitute samples for our Q-purposes. We make the assumption that if *A*, *B*, and *C* are assessed separately along Q-sorting lines, the data for all 210

statements can be thrown into one array for correlational study. Experiment shows that no important information is either gained or lost by this practice, and nothing spurious is involved.

The traits are in wide use, daily among people, and in a psychologist's "writeups." Novelists use the terms. Jane Austen, Dickens, and Anthony Trollope fill their works with characters who glide in and out of their pages as social creatures, described in such terms. We read in *Pride and Prejudice*, for example, that Mr. Bingley was "good-looking and gentlemanlike; he had a pleasant countenance, and easy, unaffected manners." He was, besides, "lively and unreserved." No intimate self-notions are at issue of the kind that enter George Eliot's novels; all is a Roget's *Thesaurus* of socialized descriptions.

Even so, any person can offer some kind of description of what he thinks his personality is like in terms of such traits, along Q-technique lines. It is true that what he thinks of himself may differ markedly from what his wife thinks he is like, or his best friend, or his associates. But all such conceptions can be reduced to direct operations in Q-technique. Even Mr. Bingley could have offered an account of himself in terms of our 210 Cattell traits, and Mr. and Mrs. Bennett and their lovely daughters could have described him, too, as Darcy and every other character in *Pride and Prejudice* could have done, all in terms of the 210 traits.

It will be supposed by many, of course, that all such self-descriptions must be biased and therefore unreliable and unworthy of scientific regard. We can accept the premise, but not the conclusions. On the contrary, precisely because we cannot believe what X says about himself, this is something worthy of our scientific regard. We surmise that the study of such self-descriptions will provide evidence of many facets of conduct and that we shall discover X in the act of rationalizing, defending himself, identifying himself with others, and much else of the kind. In the social setting, imitation, functional penetration, negativism, and much else of the kind can be reached by way of these innocent-looking self-descriptions. Concrete psychology is at issue, very different from the pseudo-scientific work with these traits in the past, where it seems that the concern was with a region of pure, abstract, unlived (and unlivable), *generalized* behaviors.

FACTOR STUDIES WITH THE CATTELL SAMPLES

We have nothing quite so interesting as a factor study of Mr. Bennett and his milieu to offer, but some of the methodological matters at issue can be outlined in terms of a study we have available of some unrelated individuals. We are to look at a table of correlations for 21 women, each of whom described herself as she conceived herself to be *usually*. The 21 women were as follows:

a) Six were *underachiever* students (Nos. 1–6), already referred to in chapter ix.
b) Six were *overachiever* students (Nos. 7–12).
c) Four were clerical assistants at the Educational Testing Service (Nos. 14–17); they represent *any* women we cared to introduce into our matrix.
d) Two were well known to the author and constitute "reference values" in the study, against which to judge its over-all validity. One was the author's wife (No. 18), and the other was a research assistant who helped him at the Educational Testing Service.
e) Three were "cases," also well known to the author because they had been under his observation, and to some extent care, for some years. One was a near-psychotic (No. 21), and the other two neurotics, one obsessional and the other hysterical (Nos. 19 and 20, respectively). The descriptions for these cases were provided by the author.

The dependency nature of this variate design will be at once apparent: the variates at *d* and *e*, in particular, as well as at *a* and *b*,

TABLE 1

	Most Characteristic									Least Characteristic		
Score	10	9	8	7	6	5	4	3	2	1	0	
Frequency	2	3	4	8	11	14	11	8	4	3	2	($n = 70$)

are clearly not indifferent matters but are likely to govern the subsequent factor analysis.

The self-descriptions were all made on the basis of the frequency distribution in Table 1. The intercorrelations for these 21 women are given in Table 2 (n = 210 traits). The (21 × 21) table was factored (centroid), providing four factors I, II, III, and IV. They were rotated to reach the simplest structure, and the outcome is the set of factors A, B, C, D. Following our usual practice, these factors are shown again in Table 3 in which only the significantly high loadings are indicated with an x. Table 3 also shows the *types* of persons in terms of their factor compositions. Four "pure" types

TABLE 2

For 21 Women, n = 210 Cattell Behavior Traits: Factors Are Orthogonal, Rotated to Simple Structure

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	Factors					
																						A	B	C	D		
1	—	250	214	105	363	297	288	387	203	446	310	−089	173	282	282	199	196	117	−042	−112	−242	17	59	18	−35	1	
2		—	175	581	211	+167	541	421	335	213	−081	085	301	464	189	382	298	296	−101	−097	−261	59	03	47	−03	2	
3			—	173	015	081	170	−050	029	100	092	243	077	139	163	243	222	095	−163	142	−009	05	−01	20	−41	3	Underachiever women (UA)
4				—	266	039	434	463	436	347	014	220	287	450	255	242	306	253	−117	−141	−018	47	05	57	08	4	
5					—	339	201	425	288	514	087	−134	216	281	378	−090	161	092	233	−097	132	−02	60	32	09	5	
6						—	045	173	007	215	154	−207	−004	206	206	122	−001	−119	141	−024	047	02	43	−01	−11	6	
7								348	263	290	−069	114	297	428	141	317	282	380	−215	−093	−201	51	02	46	−07	7	
8								—	311	444	178	−249	255	511	248	129	223	032	−003	−389	−281	52	55	22	18	8	
9									—	478	136	263	317	167	437	010	260	332	−044	−116	134	06	22	63	10	9	Overachiever women (OA)
10										—	172	130	329	220	475	−007	301	243	−023	−216	179	−02	56	56	01	10	
11											—	−051	108	229	237	207	051	−096	053	025	−097	−02	39	−03	−31	11	
12												—	263	−205	107	−041	107	354	−164	255	240	−21	−38	50	−08	12	
13													—	059	232	188	297	413	−108	−017	059	07	05	56	−08	13	Research assistant
14														—	113	487	316	037	−045	−166	−384	66	35	08	−23	14	
15															—	049	077	197	080	−221	306	−15	47	49	06	15	Clerical assistants
16																—	384	288	−108	113	−421	45	−02	10	−54	16	
17																	—	311	−136	162	−111	23	−04	32	−33	17	
18																		—	−206	−198	082	06	−22	61	−13	18	M. B. S.
19																			—	140	208	−26	22	−14	13	19	
20																				—	027	−30	−30	03	−38	20	"Cases"
21																					—	−59	00	29	27	21	

are indicated, called A, B, C, and D, respectively, and four "mixed" types:

AB: No. 8
AC: Nos. 2, 4, 7
AD−: No. 16
BC: Nos. 10, 15
No types: Nos. 11, 17, 19, 20

It is impossible here to enter into all the detail that is required for full understanding of these data. We shall take factor-arrays for

TABLE 3

		Factors				Types				
		A	B	C	D	"Pure"				"Mixed"
UA	1		x				*B*			
	2	x		x						*AC*
	3				$(x)-$				*D−*	
	4	x		x						*AC*
	5		x				*B*			
	6		(x)				(*B*)			
OA	7	x		x						*AC*
	8	x	x							*AB*
	9			x				*C*		
	10		x	x						*BC*
	11						...	(Nil)	...	Nil
	12			x				*C*		
Assistants	13			x				*C*		
	14	x				*A*				
	15		x	x						*BC*
	16	x			$x-$					*AD−*
	17						...	(Nil)	...	Nil
	18			x				*C*		
"Cases"	19						...	(Nil)	...	Nil
	20						...	(Nil)	...	Nil
	21	$x-$				A−				

granted, for example, and merely indicate the kind of considerations at issue in such a Q-study.

The under- and overachievers were introduced, of course, as a prima facie basis for personality differences: it is reasonable to suppose that some overachievers will be serious-minded, hard-working, conscientious persons and that, likewise, some underachievers will be lazy good-for-nothings or the like.

Factor B, rather than any other, suggests itself as related to underachievement, as factor C does to overachievement. The factor-array for B is striking for the extreme position given to the traits of *self-sufficiency*, *independence*, and *individualism:* these qualities are rated highest of all in the array.

But it is patent that very different matters are really involved in these women of type B. Defiance and defensiveness are at issue, and nothing essentially mature or self-sufficient. The well-adjusted student, indeed, is more modest about her self-sufficiency and independence of mind and appraises many other qualities higher than these. Yet even the defiance, which one would think of as a reaction to the failure at college of these women Nos. 1, 5, and 6, takes different forms in the three cases. Our own observations on No. 6 (as well as discreet inquiries at her college) show her to be impulsive and rebellious, "stupid" was her own description of herself—"mother doesn't know how to deal with me." She was in serious trouble at college on moral grounds and lived wildly on week ends with a depressive poet. The self-sufficiency she prized so much in her self-description was essentially a pathetic naughtiness and wild defiance set against a background of high spirits and animal liveliness. No. 1, on the contrary, was noted as dull, dour, unimaginative, crudely undeveloped; her father was an habitual drunkard and had been out of work for two years—information given by the student in dull submission and deep hurt. Either dour obstinacy or a deep wish is at issue in her "self-sufficiency." No. 5 was different again: she was noted as vain, aggressive, outgoing—she had none of the liveliness of No. 6 but also none of the cowlike apathy of No. 1. She was "hard," quarrelsome, forthright, domineering. Her independence and self-sufficiency, qua factor B, are again essentially defiance. No. 6, then, with her lively naughtiness or worse, No. 1 with her dour resistance, and No. 5 with her blunt forthrightness describe themselves as essentially alike: and so they are, but for a dynamic reason. All knew they were failures, and this *aspect* of personality, *factor B*, represents their reaction to the situation.

We suspect that many factors in Q-technique, in this region of study of the so-called "generalized" traits, will be façades or poses of this kind, the consequences of interactional situations and not inherent or fixed qualities of the women. The underachiever, No. 3

(type $D-$) is interesting in this connection. The factor-array indicates (positively) a ruthless, spiteful, wilful, "hard," and somewhat mean personality. This is rejected by No. 3: the traits are placed significantly, but at the *bottom* end of her self-appraisal (whence the negative loading in the factor)! Our own observations about her indicated a tight-lipped, withdrawn, spiteful, caustic sort of personality, too prim and proper to be genuine in the circumstances. Apropos of nothing, for example, in her thin voice and thin demeanor, she remarked that she "doesn't like emotional scenes." Obviously, her reaction is a rejection of her "real" self, which is the *positive* aspect of the factor in which she gives herself a negative loading. She turns herself about and *denies* the self-qualities "really" there. Yet even the latter qualities are reaction-formations, we are pretty certain, to the alarming situation in which she found herself as a failure at college. It would not be difficult to bring other aspects of her personality to light, far removed from the apparently "real" spite and tart bitterness she is here so anxious to hide.

The underachievers Nos. 2 and 4 and overachiever No. 7 (type AC) give an account of themselves, as shown by their factor-array, as normal, cheerful, good-natured, indulgent, practical-minded, obliging, leisurely individuals—the reverse, however, of giddy, and not shy, nervy, eccentric, or domineering. Bouncing good humor characterizes them, we might say. The underachievers had merely failed in chemistry, a not unlikely event for a woman; they apparently took a realistic view of the situation, however, and their self-descriptions, qua type AC, suggest a balanced state of affairs. But even here it is difficult to believe that these women were essentially alike—very different dynamics were undoubtedly at work in each case, producing the good-natured exterior with which they confront us. No. 2 was bland, a sickly, pale-faced young person; No. 4, contrariwise, was big, plump, boisterous; both were relatively naïve, essentially unsophisticated girls, one with and the other without good spirits. No. 7 was different again—one had only to see her to have an obvious impression of scatterbrained precocity and shallow sophistication, masquerading here as self-described "bounce" and good humor.

But it might be objected that, of course, women's self-appraisals will be of this topsy-turvy kind—who would have thought otherwise?

In Cattell's R-technique study, it will be remembered, each woman was assessed by many *others*, so that the R-factors discussed by Cattell are not self-appraisals and were regarded as "objective" and not open to dynamic mechanisms of the kind we are considering. It is easy to show, however, that, even under such conditions, different colleagues or fellow-students may grasp different aspects of a person's personality, and stereotypes are almost everywhere apparent in such circumstances.

What proof, however, is there of this topsy-turvyism? And what is to be regarded as "real" or "inherent" or "not a reaction-formation or the like"? The questions are not, in fact, immediately pertinent. For factors such as A, B, C, and D, or their combinations, concern *communality* only. The factors in R-technique are similarly restricted. In Q-technique the variance accounted for in this way is usually only about 50 per cent of what is available for explanation and study. As every factorist knows, the other 50 per cent may well be accounted for as *specificity*, and, whereas it has been usual to disregard the latter in R-technique (except for mention of it by Spearman and many reminders by Sir. G. H. Thompson of its possible importance), no such liberties can be taken in Q-technique. Specificities in Q are often of the utmost importance. Thus *the "whole" personality at issue, in the case of any of our women's self-appraisals, is represented not by any factors A, B, C, or D alone, or by their combination, but by* ALL *the initial variance, specificity as well as communality*. Factors, pure or in combination, merely represent aspects of this initial whole. It is in this sense that Allport (6) was correct to regard personality as unique, for the specificities make it so.

The magnitude of the specificity can be gathered from the following simple example:

Woman No. 13, of type C, had *reliability* (r_{xx}) 0.87 (she repeated her self-appraisal a few hours apart). The communality (r^2_{xc}) accounted for in Table 2 is 0.33. The specificity (r_{xs}) is therefore as follows:

$$r^2_{xs} = r_{xx} - r^2_{xc}$$

$$= 0.87 - 0.33 = 0.54\ ;$$

$$r_{xs} = 0.73\ .$$

This is larger than any of her loadings on the factors A, B, C, and D, and, indeed, larger than anything we can make for her by way of rotation. If one aspect of her is "explained" by factor C, we have more variance still to "explain" by way of her specificity.

The consequences of this are important to the whole framework of Q-technique in relation to personality theory. In the last analysis, communality and specificity factors are obvious abstractions, but the specificity, rather than the communality, could *characterize* the personality.

Consider, for example, type C. The factor-array for C indicates a pleasant, serious-minded, and well-adjusted personality: a sense of breeding is indicated, of responsibility and conscientiousness. The women involved regarded themselves as companionable, but not widely sociable, as sober-minded, somewhat self-effacing. All of which may be "real" enough: indeed, it seems scarcely necessary to look behind this screen for reaction-formations, defense mechanisms, or other dynamic influences. Certainly, these women impress one by their good breeding and good sense. But the over-all descriptions that the women give themselves are very distinctive. No. 9, for example, describes herself as essentially reticent, silent, and indeed somewhat frightened, even hurt (indeed, she was, for her highly respected elderly father had recently run away with a young secretary, with deep hurt to this daughter and to an equally withdrawing mother). No. 12 described herself as soft-hearted and good-natured, in a quiet way. No. 13 portrayed a harder, more manly quality. No. 18 had quite different qualities running through the undercurrent of factor C. *These* are the "essences" that the women expressed as characteristic of them, and not merely the quiet breeding of factor C.

The example of superimposed photography is again worth mentioning in this connection. *Communality* factors are to be compared to composite portraits. Relative to these, however, a *specificity* (since it is reliable) gives the portrait of an individual its verisimilitude, its true-to-life appearance. This, we add, is an empirical finding, not a *necessary* consequence of any factor analysis.

But the subject can scarcely be aware of these differences between the aspects of her personality represented by communality, on the one hand, and by specificities, on the other. The communali-

ties depend, moreover, upon the *other* persons who happen to be placed in the correlation table as well. However, in so far as any person can be placed in one type or another, that is, in so far as communality obtains for the person, to that extent its specificity can be expected to be indicative of something peculiarly characteristic of the person. Persons 11, 17, 19, and 20 (Table 2) were of no type in the table concerned; along with persons like themselves, however, their types should become apparent and consequently their specificities as well. It is theoretically possible for a person to be so unique that no other person can be found to correlate with him; in this case all the variance will be specificity.

We make no attempt at this point to determine how many clearly discernible *types* exist of the kind indicated in Table 2. The number could be large. An important methodological question arises, however, about the relationships between *pure* and *mixed* types, which was discussed earlier (p. 41).

Meanwhile, a useful empirical discovery has been made, to which we draw attention, that it may be *specificity* that gives a personality its uniqueness, its special "character," as this is outwardly manifested.

Before leaving Table 2, two details are worth notice. There are clear indications of reaction-formations for type *C*, which seemed at first sight to be innocent of anything of the kind. No. 13, for example, undoubtedly accentuates a certain analytical-mindedness and cold rationality of outlook, in keeping with her research milieu but out of gear with a very charming and natural interest in man-hunting expeditions. Other aspects of her personality, we feel sure, could be brought to light very readily. A beautiful case of a dynamic Adlerian inferiority compensation is shown by No. 11, who conforms to no type in Table 1. All her variance, in short, is specificity (relative to the matrix). She describes herself as highly intellectual, with a keen analytical mind, a fine imagination, and generous. In fact, she is a cripple, with a lovely face, who expressed open jealousy of her fellow-students and whose personality could be characterized only as softly and constitutionally inferior. She is really somewhat dull, as the scholastic aptitude tests had originally indicated; but she works hard. The undercurrents of resentment and compensation for organ inferiority were patent for almost anyone to see.

A number of important points have now been made, of considerable methodological interest. The Q-approach, obviously, can bring dynamic matters to light which are necessarily ignored in R, or which are regarded as "subjective" or "errors" or the like relative to the supposedly inherent principles looked for at levels of high abstraction and generalization in that technique. But attention is also drawn to mistakes that will be made if communalities alone are attended to, oblivious of the sometimes more important specificities. Indeed, cases Nos. 11, 17, 19, and 20 in Table 2 can have no explanation, in the present context, except in terms of their non-communality variance; yet real enough personalities are obviously at issue. In all the other cases there is more left over as specificity than is accounted for by communality. To suppose that specificities are of no scientific value, because of their apparent uniqueness, is nonsense.[1]

Finally, one may begin to see why the supposed connections between R and Q, even for the same nominal traits such as Cattell's, is a pseudo-problem only. None of Cattell's, Burt's, or Thurstone's R-factors have ever reached into matters of the kind we have been discussing, where dynamic processes are everywhere apparent; nor could they ever do so if wholly generalized situations are postulated and a belief in universal propositions everywhere implied.

There issues, then, a region of study which is open to us for the first time, concerning the relationships that exist between the factors of a person's *self-notions* (chap. xi) and the same person's more socialized personality, such as we have been considering. But this we must leave for the future to unfold.

ASPECTS OF PERSONALITY

Not only were the R-factorists wrong about the importance of "subjective" data, but it is not difficult to show that, even from the external frame of reference, very different aspects of a personality may be grasped by onlookers. Clearly, the same person may exhibit very different personalities in different interactional settings or may be seen in very different lights by those around him. Gyges, ancestor

1. How important specificities can be in interview studies is obvious. The interviewer can often grasp the specificity of the person he interviews, and nothing of any communality.

of Croesus the Lydian (in Plato's *Gorgias*) was one personality as a shepherd and a totally different creature as an invisible philanderer. "He was all right," his friends might have said, "as long as he kept away from the women." Two aspects of his personality are here contrasted, and most of us, perhaps, are many-sided in the same fashion, displaying one aspect of ourselves at home and another at work or play. It is difficult to understand why anyone should believe that only what is *general* to such differences should have prior claim to scientific attention, as R-factorists appear to believe. On the contrary, it would be wise to study a person, as one might Gyges, in his concrete settings, since the very changes from one setting to another may provide crucial evidence about his personality. However, we cannot follow a modern Gyges around. A study of the *interview* situation can provide us, however, with all that we require in this connection, for it is easy to show that different interviewers may grasp quite different aspects of a given personality.

SOME DEFINITIONS

It will be helpful to state a few definitions at this juncture: in the studies which were referred to in the previous section each factor was interpreted as an *aspect* of a prior whole self-description. This prior *whole*, of course, is operationally defined in terms of what the person provides as his self-description—it has no other significance for us than this. Moreover, there could be many different *wholes* of this kind for any person in different interactional settings. It has been shown that the *specificity* factor or aspect of a prior *whole* is not less essential for understanding a personality than is any of the much-vaunted *communality* aspects to which prior attention is likely to be given on the grounds of their "greater generality." When we turn to study the Q-technique descriptions by *observers* about the personality of another person, X, whom they observe from the external frame of reference (our probe *b* [i]), each description *about* X is likewise a prior whole, which may be divisible into whole aspects of various kinds, including the *observer's* specificity. There has been much talk in the past, of course, about the "whole" being greater than its parts and about the spurious nature of much, if not all, analysis of such wholes into parts; in Q-technique we find no difficulties about such issues. If we wish to know what a person thinks

he is like, or what he thinks another is like, then certainly these are represented by the prior *whole* arrays and not just by any communality factors we might wish to discuss for or about him. It is such wholes that we have to explain. But it seems quite easy to do so and to retain the essential uniqueness of the personality, in terms of communality factors and specifics or of the different whole aspects that may be grasped about X by himself or by others. Thus we saw that there was nothing incompatible about regarding three women as alike with respect to type *C* above, and yet very different personalities when their specificities are taken into account as well.

However, we might wonder how far outside observers can grasp X's personality, whether as a whole or with respect to its aspects or its specificities, and under what conditions. An interesting suggestion has been made (62), to the effect that *functional interaction* with X is important in the latter connection. Studies of clinical judgment, such as Weingarter (198) describes, are clearly open to Q-technique procedures, as are all that have reference to the way any person conceives or understands any other.

A VIEWPOINT ABOUT PERSONALITY

There are other branches of our subject that should be touched upon, concerning the general questions of how we deal with *theories* of personality and how we propose to experiment upon the so-called "processes" of personality (such as the defense mechanisms of psychoanalysis). We must leave these for later treatment, although hints have already been given about both. The prototype for the study of processes is provided by the *selective service* problem in chapter vii. With respect to theories, almost any may have its independencies represented in structured samples, as has been described for Jung's type-psychology. We have devised samples for Glover's treatment of the psychoanalytic theory of "character formation" (165) and for Erich Fromm's theory of *productive*, *hoarding*, *marketing*, *receptive*, and *exploitative* types of American men. Bion's theories have been represented, likewise, in simply structured Q-samples (133). However, it is what one does with the samples that really matters, and, for an excellent example of such experimental possibilities, we recommend the study of Edelson and Jones (62), in which self-theory was at issue, of the kind dealt with

by Rogers, Snygg and Combs, and others. Their study was notable in several respects, for their bold experimentation in a postulatory-dependency manner and their collection of a sample of self-referent statements. Much of this work must await later appraisal. We have provided a simple design, elsewhere (174), for testing theories such as Allport's (6) on functional autonomy.

It is clear, therefore, that we have much to offer in the various fields of personality study. A more adequate treatment of the whole subject, indeed, is planned from the methodological standpoint. Meanwhile, we suspect that psychoanalytic and self-theory orientations about the subject have most to commend them for future experimental purposes. But it is permissible, perhaps, to offer as an orientative matter a viewpoint about personality which might at least be refreshing in a field that has become cluttered with learning theories, need satisfactions, trait clusterings, behavioral phenomenology, and much else besides. Our standpoint will have the merit of being concerned with testable propositions and with, essentially, an orientation toward the making of discoveries and not merely toward the explanation of what is well known already.

It is helpful to keep our objectives clear: much that goes on in the name of the scientific study of personality has to do with the rather special exigencies of life, in a clinic, school, industry, or the armed forces. Sight has been lost of the wider interests in personality, such as are covered in biographies, novels, and the workaday world of religion, politics, acting, and the home. Who is there who is not interested, after all, in the personality of a Queen Elizabeth, a Rita Hayworth, a MacArthur, a Nehru, a Gandhi, a De Gaulle, a Truman, or a Winston Churchill? Biographers never seem to find it difficult to write about such persons, at best with wisdom and penetration. But one shudders to think what an R-factorist or a learning theorist or a phenomenologist would do with the same personalities in the name of psychological science. Yet, whatever else personality is concerned with, it should surely be with something which is characteristic of a concrete person. We now see clearly that this is not the same thing as an account of the vast assemblage of personal qualities that are such a person's capabilities, potentialities, and the like, for these have no better status than his bankbook, his motorcar, or his other worldly possessions. It is what

he does with such possessions and dispositions, if anything, that might be of interest for a study of his personality. Personality is to be found within the Ciceronian rubrics of *one's self*, the *part* one plays in life, and the *distinctive quality* that one has that lifts one above the common mass. There is something here that seems to be on the right lines. The concern has to be with series of Q-studies on self-notions, with aspects of personality, personal status (Burgess, 33), and cultural stereotypes. We should use what techniques we have to study persons as such, in all their uniqueness and distinctiveness, as Allport (6) has long suggested. Can we beat the novelists and biographers at their own game?—this should be the measure of our success, and it should be done with as much penetration as psychoanalysis has always insinuated for itself, but in our case along lines of testable propositions and Q-methodology.

Nor are we suggesting an easy way to beat the Dale Carnegies and other self-help writers about personality at the important game of interpreting psychology to all and sundry, as a social duty or for personal gain. Rather it is to inform ourselves, as psychologists, no less than those who would be interested in our findings, about the qualities of ordinary men and women, as well as of great ones. Can we offer anything new, for example, about the personality of a President Truman? That he has the "health-giving qualities of temperament," those of cheerfulness, courage, and stubborn resiliency, is no doubt true enough—but the reporters have noticed this also. That he works hard, at a lively pace, is exuberant, and proud and jealous of his country, family, and calling—this is all known to journalists and music critics alike. He is a man of character, in the old-fashioned British sense, a man with firm loyalties to his country first, and to the world perhaps no less than to his own family and intimate friends: biographers will no doubt attend to such matters. But whence came the voice of his personal courage and the way he stretched up upon his toes (as the journalist, John Hersey, has put it) for a sight of famous people, from which he, an American "little man," sought to match the full dignity and responsibilities of his important place in the world? It would be fascinating indeed to have the honor of studying such a man from his internal frame of reference, to let the wide world know, one day, what is really involved psychologically in his personality. Nothing that the

scientific study of personality has touched upon, up to now, can help us very much to formulate on theoretical grounds what is likely to be at issue. No doubt psychoanalysts will make a prodding at it; but they would miss the sublimations. Yet the means now exist for studies about such a personality, or any, and this without reference to what others think or believe about him or them. And what would be testable? We would refer, for an answer, to chapter xi. The propositions which are worth testing can be best suggested by the person's own history or by the factors inductively arrived at from a study of the individual's self-notions. These would be sufficient to set us going.

Meanwhile, this chapter has provided some very simple examples of the tools now at our command for such studies. To these must be added whatever is possible for the study of self-notions. But the study of *traits*, regarded as mere "meanings" in a dictionary sense and never as behavioral segments with "general implications," is of considerable interest. We suspect that highly *social* matters are really at issue for such traits, in spite of calling them "traits of temperament." We suspect, also, that much more can be learned about personality from a careful exploration of a few really interesting people, such as a Kierkegaard (95), a Churchill, or an Einstein. We are by no means satisfied that theories of *needs*, *drives*, *enduring egos*, or the like are what the present occasion requires for the direct study of what it is that, in Cicero's words, raises a man above his peers.

CHAPTER XIII

Q-TECHNIQUE AND THE PROJECTIVE TESTS

THE PROJECT

THE so-called "projective" tests, such as the Rorschach (132), the *Thematic Apperception Test* (118), and the like, are of particular interest to us. For a long time these have been the target of criticisms on the grounds of their unscientific procedures: it seems that the projective testers look back, somewhat illegitimately, like Lot's wife, at the salt of profligate speculation. Our own view of the matter is that these tests are the most interesting, scientifically, of all that psychology has available; this is because they reach into the *actual*, rather than the merely possible or *potential* characteristics of persons. The critics were thinking of the latter and of nomothetic matters in general; the clinicians were interested in the former and in particular idiographic implications. The critics, of course, had a sound methodology for studying the former, and they wrongly thought that this alone offered the necessary foundations for the scientific exploration of these tests. The projective testers did not quite know what was wrong but felt strongly, nevertheless, that in some way they were on the right lines in spite of the critics. It is our purpose to indicate that their faith was justified and their devotion not misplaced.

We shall deal with the Rorschach test first and then with the TAT, the now-familiar pseudonym for the *Thematic Apperception Tests*, which Murray (118) so auspiciously revived for psychology.

PROJECTION

By "projection," in psychology, we commonly mean that one person imputes to another some notions that really refer to himself: X is guilty or stingy, but he projects this more or less unconsciously upon others around him and thinks that they are blameworthy and mean. It is certain that, under conditions of stress and

mental conflict, men, women, and children are likely to project in this crude and direct fashion. The *Thematic Apperception Test* is particularly valuable because the relatively unstructured pictures it employs make it easy for the distressed individual to project upon them his own preoccupations: the woman who is having serious marital trouble "gives it away" by making references, in her responses, to a woman in the pictures who is having quarrels and unhappy episodes with a husband. This is usually done with little or no awareness that these are revelations with obvious pertinency to her own case; only rarely, indeed, does the subject, upon reflection, notice that she has unwittingly disclosed matters that she would have preferred, perhaps, to keep to herself. We call projection of this kind "functional" or "reactional," and it is in some such sense of the term that the psychoanalysts first employed it.

But the TAT and Rorschach theorist uses the term "projection," usually, to mean more than this: some responses of the person are supposed to be *habituated*, and from these the psychologist draws inferences about the individual's personality in general, his intelligence, emotional stability, and the like. The person "gives away" his personality characteristics, projecting these in the sense that Mercator's projection represents the earth on a map. Thus the Rorschach is not likely to touch upon reactional projections but deals instead with what might be called "structural" projection: that is, with habitual "forms" of behavior or response—the person has a dispositional tendency to grasp wholes rather than to be concerned with details; or he is pedantic and shows it in his reaction to the Rorschach; or he is introverted, thinking usually of people and their doings, and hence his responses tend largely to be *M* in the Rorschach. Or, if analytical interpretations are considered to be pertinent, the responses are looked upon as manifest content, whose latent content has to be reached by way of association—the Rorschach abounds in sexual symbols, and this is quite a possibility. Thus the *functional* projections are likely to concern present difficulties or the like, or offer a pointer to "deeper layers" (as it is called) of the personality; but the *structural* may deal with habituated epitomes or condensations of personality and behavior.

Q-technique can provide means for testing propositions as to whether reactional or structural matters are at issue. Some notable attempts have been made in recent years, by Sears in particular

(141), to reduce projection to testable form. Sears argued that students who are actually stingy will tend to impute stinginess to others around them: he had students assess one another and found little that was significant, except that the stingy students seemed to attribute *more* of generosity to their friends than they deserved, and *less* of stinginess! The mean, indeed, are generous at heart; and the generous, no doubt, essentially mean at heart. Sears has rejected such psychoanalytical doctrines as unproved because of studies of this kind; but we have suggested elsewhere (174) that some caution is required. Sears left many methodological questions untouched which Q-technique could begin, at least, to answer; and much experimental work must be undertaken along Q-lines before the full import of psychoanalytical doctrine can be grasped, for it is unlikely that the behavior involved can be rejected. Readers may wish to contrast the essentially nomothetic approach to these matters by Sears, who studied three or four *traits* for a large number of persons, with the Q-technique approach recommended in its place by us (174), in which a large number of traits are studied for one person, or for a few only. Rather complicated experimental designs are involved; but the proposed studies of projection along Q-lines would be sounder for many obvious reasons.

PRELIMINARY METHODOLOGICAL CONSIDERATIONS

The Rorschach test, it scarcely needs saying, consists of ten ink-blots to which the individual responds, saying what he "sees" in them, much as he sees things in a fire or the clouds. It is supposed to provide information about personality as a *whole* (Beck, 16; Klopfer and Kelley, 96). Prior to the onrush of interest in the TAT, which is of recent origin, the popularity of the Rorschach had increased by leaps and bounds. Bell's bibliography on the test runs into nearly 800 references (19) up to 1948. It has become fashionable, too, to suppose that the field of perception offers a common point of orientation for all psychology (Murphy, 115; Roe, 128; and others), and, since the Rorschach happily involves visual percepts, its value has become enhanced accordingly. Not only is there guilt by association but also enhancement by a verbal trick. The Rorschach's severest critics have been, perhaps, Thurstone (190), Zubin (210), and Cattell (49), each of whom objects to the many unverified assumptions upon which the testers base their interpretations.

Scientific work on the Rorschach has consisted very largely of studies about its reliability and validity. Hertz (79), Vernon (195), Thornton and Guilford (184), Troup (193), Ford (69), and others have dealt with the test's reliability from various angles, and studies of its validity are numerous. The latter have been along two main lines, the one retaining a certain "personality-as-a-whole" framework, whereas the other has been rather more analytical in intention and has presumably concerned separate processes or the like of personality. What was intended in the two cases was no doubt on the right lines, but the methods employed for their study have, we shall indicate, some severe shortcomings and weaknesses which it is the purpose of Q-methodology to obviate.

From among the scores of references on validity study of the Rorschach we would draw attention to the following methods in wide use:

i) A favorite method has been to compare blind *diagnoses* on the Rorschach with diagnoses made by clinicians, as in the studies of the Buhlers (37), Garfield (72), Benjamin and Ebaugh (20).

ii) *Personality sketches* based upon the Rorschach have been matched against those made from clinical observation, using Vernon's matching coefficient (195).

These two deal with personality-as-a-whole. The more analytical methods may be typified by the following:

iii) The attempt has been made to validate the separate determinants of the Rorschach by correlating them with variates presumed to measure the same process. Thus Thornton and Guilford (184) found no relationship between introversion-extroversion as measured by an *inventory*, and the same apparent process according to Rorschach indicators *M*, etc. On the other hand, intelligence assessments taken from the Rorschach appear to correlate with measurement of I.Q. for the same persons (Vernon, 195; Beck, 16; Ford, 69; and others).

iv) Recently, factor analysis has been resorted to by Hsü (85), Wittenborn (201, 202), and Sen (142). Wittenborn determined the frequency with which a group of subjects gave *W*, *S*, *Dd*, *R*, *Fc*, *FC*, *F*, etc., responses in their Rorschach records. These constituted variates, which were correlated and factored. He found four factors for the main part: (*a*) one covering *W*, pure *C*, shading, and texture; (*b*) another subsuming *M*, *FM*, *FC*, and *D;* (*c*) one for *Dd* and *R;* and (*d*) another for *S* and *O* responses. Sen's study followed the same lines, but she named her factors *fluency of association* (comparable perhaps to *c* above), *general intelligence* (*a* above), *emotional introversion* (*M* responses, etc.), and *associative reproduction* of particulars (*F*+, etc.).

The concern in methods i and ii is with the proposition that a personality description made from the Rorschach tallies with a validating one made from wider clinical observations. (It is doubtful whether a *diagnosis* corresponds to a *description or sketch* methodologically, but we give method i the benefit of the doubt for the moment.) The proposition is inadequate for a number of reasons.

First, the Rorschach could conceivably probe into matters that clinical observation alone may fail to reach. If the test can only do what clinical observation does, its use becomes one of mere convenience and not one of *necessity*. Second, even if agreement is found between the test and clinical descriptions, we do not know to what it is due. There is no compelling evidence that expertness is being tested, of either the clinician or the Rorschach tester—similar results might be reached, perhaps, by any intelligent person. Or the agreement could be due to somewhat obvious or superficial matters, without touching upon theoretical issues of importance. Third, the methods have no analytical implications. This is not to say that one should not be concerned with such global aggregates as "personality-as-a-whole." But these studies provide merely "dead-end" facts showing, perhaps, that for 80 per cent of the cases Rorschach and clinicians agree, and for 20 per cent of them they disagree. Nothing *operationally* defined can be referred to, by which we indicate why there is this agreement or disagreement. We might, of course, make guesses or have hypotheses about these matters, but it is another matter to have operations upon which to examine them concretely and objectively. The necessity for such operations has probably not occurred to the researchers concerned, and perhaps could not do so without knowledge of what can be achieved in this direction along Q-methodological lines. Fourth, since a clinical approach is at issue, it is surely scarcely adequate to have to support one's prowess "on the average" if methods exist for dealing with each case concretely and individually. A physician, of course, may make diagnoses about pneumonia with less then perfect adequacy; but he is, after all, merely inexpert—a practitioner and not a researcher. Research on personality should be somewhat more sophisticated in its objectives.

In spite of these limitations, there is some realization, in these descriptive studies, that personality has a certain "whole" aspect or

quality. Nothing is more in need of logical analysis than the concept of "personality-as-a-whole"; and we cannot attempt this here. But it seems to us that it is from this standpoint that personality study must *begin.* As Ward (197) long ago taught so eloquently, the concern is with a certain "synthesis," a "concrete personality." The essential matters at issue, we are sure, are missed by such critics of "wholeness" as Cronbach (55). But we can attend to these more appositely, perhaps, when some account has been given of the analytical procedures, represented typically by methods iii and iv above.

The concern now is with the separate classes or categories of response to the Rorschach, studied frankly with respect to *individual differences.* Each class of response or datum (such as *W* responses, or total number of responses) becomes a *variate,* and the inquiries are directed toward studying their interrelationships (as in factor analysis at iv) or validating each, if possible, as a separate function. All such studies, methodologically regarded, come within the rubric of R-methodology.

Now the tests or variates of R-methodology operate, in principle, according to the "rule of the single variable." Each test is a separate affair, and, in principle (as in Thurstone's work, 188), one tries to construct each so that it will measure a single function as far as possible, under conditions made as favorable as possible to the subject. It is probably this that lends support to the fact that the methodology has always been linked to the study of *capacities* and *potentialities*. Even the term "ability" (as used by Spearman, 148, because of the innate implications of the former terms) had the meaning of a measurement made according to the rule of the single variable. A person works at only *one* test at a time. What he does at one test is not supposed to influence what he does at another (a matter for separate study, rarely undertaken in differential psychology). If, then, we are to play the game of R-methodology fairly, we must seek to measure our variates according to this rule, whatever else we may do as well. This has not been done, so far, for any studies of the Rorschach.

Very tricky matters are involved. The Rorschach is a multi-stimulation instrument. Its methodological structure is analogous with that of a balanced block design in Fisherian methodology, the

chief characteristic of which is that many independent variables mediate upon a dependent one. It is *possible* for the subject to respond to every location, determinant, or other condition of the Rorschach. However, he may give *W* responses only. This does not mean that he could not under other circumstances provide *Dd*, *S*, or *D* responses. Or, if he gives only *Dd* responses, this does not mean that he is incapable of *W*, *S*, or other responses. Under certain conditions of instruction he could undoubtedly give other types of responses as well, but these would be more or less *mechanical* and not the projected ones which, it is believed, the Rorschach insinuates upon the subject when the test is applied in the usual manner. Thus, suppose that our subject X gives no *S* responses to the Rorschach as normally applied. Having told him what an *S* response is, it is a simple matter to measure how many of these he can give in a minute, for each Rorschach card in turn. The same can be done for each category of Rorschach response, for *W*, *D*, *Dd*, *F*, *FC*, *C*, and the rest. Each would be measured according to the rule of the single variable. But what is measured this way could be very different from what these types of response indicate in the unstructured situation. Thus, when Thornton and Guilford (184) used the *Nebraska I-E Inventory* to assess the introversion-extroversion of their subjects, this was a measure according to the rule of the single variable, a potentiality for something. But the Rorschach indicators for the *Erlebnistype* do not necessarily mean such a *potentiality*.

A few years ago we conducted an experiment on the problem just discussed. The Rorschach was first applied in the ordinary way. The subjects were then told what was meant by a *W* response and were given two minutes to provide as many as they could for each of the ten blots in turn. This was repeated for *D*, *Dd*, *S*, *FC*, *C*, *F*, *Fc*, *K*, *FM*, *M*, and *m* in turn, making 12 variables in all. The data for 60 men, aged twenty-one to thirty years, was correlated and factored. Three factors were distinguished:

a) One for *D*, *Dd*, *S*, *F* and *FM*. We thought of it as "fluency of ideas."
b) One for *W*, *K*, *Fc*. We called it "sensitivity," a sort of introjected sensibility.
c) The third was centered upon *color* responses, *FC* and *C*.

These factors, we believe, are very like those adumbrated by Wittenborn and Sen. A reanalysis of Wittenborn's data by J. C. Nun-

nally (119) gives results that compare very clearly with those above (a fourth factor involves *Z*, which we did not have as a variate in our study). Sen's factors correspond also to ours, if it is seen that our *b* and her *intelligence* are comparable. These results, if supported, suggest that Wittenborn and Sen have grasped the "mechanical" responsiveness to which we have referred rather than the naïve projection, upon which, we must suppose, the test theory is founded. They have not touched upon the latter theory in any pertinent manner.

Moreover, it is impossible to accept any *functional unity* or the like for these factors. Thus three persons, X, Y, and Z, could gain the same factor score for the factor of fluency (our *a;* Wittenborn's *c;* or Sen's comparable factor), but with very different connotations. In X's case, overelaboration and circumstantiality may be at issue, a horrid pressure, indicative of anxiety; in Y's case mechanical memory may be at issue, such as one sees in a subeditor of a newspaper, who knows the answer to every question but has no insight into, or elaboration of, any. In Z's case a rich *intellectual sensitivity* may be involved. The same qualitative differences are clearly possible for each of the factors. One would have to be a very naïve scientist indeed to overlook these possibilities or to accept factors as necessarily operational definitions of single process conditions, the same for all persons, differing only in degree.

These methods of differential psychology have been recommended, nevertheless, by Thurstone (190), Cattell (49), Zubin (210), and others as essential to the scientific study of the Rorschach. Thurstone asks the question, for example, whether it is true that projective tests like the Rorschach cannot be objectively scored, as is widely supposed by clinical psychologists. His answer is that they can be so scored. To illustrate the point, he refers to a test of *homonyms* (190). Each homonym has two possible meanings, one in the human and social field and one in the physical or literal one. The first few items of such a test could proceed as follows:

Write a word having the same meaning as that given:

Function..

Address..

The first may call to mind either a social affair (*S*) or a mathematical expression (*P*); the second a speech (*S*) or a street number

(P). For a list of 40 such items it is easy to see that some people might give more of one kind of response than the other. The individual differences are likely to be considerable. But the situation is one in which, at most, two variates are assumed, one for S and one for P, the two being mutually exclusive; or else a single variate is assumed, bipolar for S and P—most people give S and P responses equally often, but a few give all S, and a few all P, the distribution ($S - P$) being more or less normal about zero difference. The situation for the Rorschach, however, is multivariate and without logical connections for the main part, and it is the subject, not the experimenter, who fixes upon those to which he will or does respond. The subject chooses his own *dependent* effects, which are not necessarily those that the experimenter may force upon him by applications of techniques and methods based upon quite different assumptions.

We have given some care to these R-method applications because they are all too likely to be regarded as models of scientific procedure. We might conclude by asking what, indeed, they can hope to achieve. That they might give some evidence about *capacities* or *potentialities* of a kind is possible. But that these are necessarily important for the scientific use of the Rorschach is a gratuitous assumption. They are more likely to be irrelevant to the projective issues. The best that can be said for them, perhaps, is that they are contributions to what may be regarded as *postulatory* to the test, a matter we shall consider later. Meanwhile, we doubt whether they raise or solve any important issues, and we deny that they have any special scientific status in relation to the Rorschach. It may be concluded, rather, that the assumptions involved in R-methodology are inadequate for the proper scientific treatment of the Rorschach test.

METHODOLOGY OF THE RORSCHACH

We propose a different approach to the scientific issues for this test, which, though sympathetic to i and ii, has wider implications and better technical resources to support it.

It will be necessary to begin with a simple example. A person, X, responds to the Rorschach and may do so in a characteristic fashion. Thus we once tested an army captain, of the 7th Hussars, who had spent some years in Germany as a prisoner of war. Outwardly self-

possessed, with the self-assurance of any young man who had been at Eton, the test showed an unusual circumstantiality and proliferation of response. Either he was brilliantly poised and intellectually sensitive in an artistic way, or else the verbiage was a defense mechanism, a foil against distressing hysterical conditions or the like. The question is, How can we set about proving the correctness, or otherwise, of our suspicions about the case?

The young captain, in point of fact, was being interviewed, when we tested him, for an important position and made such an excellent impression at the interviews that a committee placed him highest in a list of 25 applicants. We were in a minority, giving him the lowest place. He was given the appointment. Within a few months the correctness of our guess was apparent to all—the captain became depressed and was seeking psychiatric treatment. We report this not to put it forward as evidence for our assertion about the case, but to say that facts of this kind, though no doubt interesting, are of no direct consequence to our diagnosis about the captain. No doubt if we had kept our eyes on him long enough, something of the kind might have happened, and we would have presumed upon its fortuitousness. Or, if we had looked back carefully enough into his history, facts might be found which, reasonably interpreted, would have supported or denied our suspicion about his hysterical neurosis. We could perhaps set about psychoanalyzing the captain, to see whether hysterical neurosis was really at issue. But this is time-consuming, involves no testable operations, and we have no guaranty that such conditions are not found in all young men of the captain's social background, at some point or level of the analysis. Nor do we set any store upon our guess that the captain would find the new work so exacting that he would soon be in trouble—it is for personnel selectors to make such inferences and guesses as best they can. These, in any case, are a step removed from the clinical diagnosis itself. It is along none of the foregoing lines that the diagnosis should be supported. What we require, instead, is a method which will take up the case with the hints about it provided by the Rorschach and which will pursue these to their scientific conclusions.

There are probably many ways of doing this. We propose one in Q-methodological terms. It is possible to determine from the case, *in terms of his own operations*, what modes of self-referent, self-reflec-

tive, behavior he has, of the kind discussed earlier, when we referred to factors f_1, f_2, and f_3, explicable as *idealization* and the like, for a particular case. This, we suggest, offers a way for probing further into any person, with the expectancy that answers can be given to problems of the kind we have been discussing for Captain X. It is not a question of *hoping* that relevant factors will "turn up." The case can be discussed on theoretical grounds, the hypothetical issues can be presented in Fisherian factorial designs, samples of X's self-referent statements can be taken for such designs, and testable propositions can be asserted for X and put to empirical test.

In the first place, therefore, the Rorschach is to be regarded as an initiator of problems about a person. The value of the test, in the scientific sense, lies in whether it points to hypotheses of importance about a person that are not apparent by other means or not so readily reached along other lines (e.g., by clinical observation). Methods are available or must be found for testing these hypotheses concretely, with the case on our laboratory table, so to speak, probed into by Q-technique or other devices. The problems may also concern more mundane technical matters, such as determining how far, and with respect to what, different Rorschach testers agree about a case which is operationally defined. Or the concern might be with the definition of *types* operationally. Or, as in Pemberton's study, one may find one's problems in an inquiry into diagnostic skill (123). In all such studies only one person, or a mere handful, is ever at issue. It is quite impossible, here, to outline all the ramifications and procedures opened to us along these lines: for some indication of the kind of analysis now possible, one may go to the studies already referred to, by Hartley (76), Rogers (129, 130, 131), Pemberton (123), Edelson and Jones (62), and a recent paper (165) on the methodology of the projective techniques.

It is convenient to continue our exposition of Q-methodology in relation to the Rorschach by referring to some studies by Dr. Beck (17), who has used it to considerable effect in some studies on schizophrenia. Psychiatrists had drawn up a set or list of *clinical formulations* about schizophrenia. Statements were about (*a*) the patient's *defenses* (obsessions, compulsions, phobias, autistic fantasies, etc.), (*b*) his *ego-functions* (co-ordination, gestures, action, self-concept, etc.), (*c*) his *emotions* (anxiety, moods, etc.), and

(*d*) the *restitutive forces* at issue (religiosity, world reconstruction, fantasies, and the like). Dr. Beck had placed alongside these formulations the counterparts for the Rorschach. The following is an example of a psychiatric formulation and its Rorschach correlate:

Psychiatric Formulation	Rorschach Indicator
Withdrawal into autistic fantasy.	*M!* and > *C*. (The degree of withdrawal will depend upon the number and uniqueness of *M*.)

The pair may be called a "correlate." There were upward of 200 such paired statements, in whole or in part.

The R-methodologist would proceed to examine each of these paired statements as if it were a general proposition. He supposes that what is at issue is that "withdrawal into autistic fantasy" leads to the Rorschach indicators $M! > C$, either "usually," or "on the average," or "in a large proportion of cases," or the like. Norms are compiled in relation to such assumptions. Our own view is that no such "general implications" are at issue.

Sufficient has already been said above about the difficulties that attend any regard of such correlates as functionally related with respect to individual differences and "rule of the single variable" conditions. Moreover, in studying any such statements, the R-methodologist has to ignore differences in their relative importance or significance in a personality.[1] That is, there are no *operations* in R-methodology by which the various statements are compared *relative to one another*. This is basic, instead, to Q-technique. And, again, what $M! > C$ means for a case depends upon the context in which it occurs. Even if, "on the average," such an association between austistic fantasy and $M! > C$ is demonstrable, either for "rule of the single variable" or *projective* conditions, the fact is not apposite (except actuarially) to the examination of a particular case X. "On the average" is not the kind of concreteness required in clinical research.

1. When the paired statements are separately studied, the data for each have to be standardized with respect to individual differences, so discarding any information that might be contained in their *means*.

Each correlate, therefore, is to be regarded as a statement of *fact*, a postulate, or a contingent proposition. Each has to be qualified by adding to it a statement of the kind "This correlate is true under certain conditions, mainly related to X's personality-as-a-whole." The latter aspects of X's personality can be grasped, we believe, if the correlates are all employed relative to one another, as is achieved precisely in Q-technique. We never seek to prove or verify the correlates separately or directly, for any "general implications." Instead, they are *used*, like rules for a game of chess. If interesting discoveries can be made with these as premises, we shall be on the right lines scientifically.

AN EXAMPLE

Thus, given a sample of the psychiatric statements, psychiatrists, from their clinical study of a case, can offer a *description* of it as a Q-sort under specified conditions of instruction. From the Rorschach, clinical psychologists can offer Q-sort descriptions of the same case, using the same set of psychiatric statements. It is then a simple matter to correlate the psychiatrists' and the Rorschach descriptions, to see how far they are in agreement.

Thus three Rorschach testers, X, Y, and Z, and a psychiatrist each gave a description of a case P. 1, the former by blind analysis, and the psychiatrist from face-to-face interviews with P. 1. They used a sample of 160 statements, taken from the psychiatric formulations discussed above, for the distribution of scores given in Table 1. Their correlations are shown in Table 2. It might

TABLE 1

Score	10	9	8	7	6	5	4	3	2	1	0	($n = 160$)
Frequency	6	8	14	18	22	24	22	18	14	8	6	

seem that some of these correlations are very low. But in factor work we learn to suspend hasty judgments of this kind, until information is at hand about the homogeneity of the sample and the kind of factors at issue. Thus it may be highly significant that X correlates low in this table with the others—his appraisal may be for something not grasped by the others. (We should add that the reliability of all such Q-descriptions is at least

of the order of 0.80, more than sufficient for communality requirements.)

The data for Table 2 are next factored. If only one factor is considered adequate, the loadings for it are as follows:

	Factor I Loading
X	0.30
Y	0.84
Z	0.84
Psychiatrist	0.62

These, when cross-multiplied, provide the "hypothetical" correlations in Table 3. These values differ little from those in Table 2. A

TABLE 2

CORRELATION COEFFICIENTS FOR $n = 160$ STATEMENTS
(Appraisals Are for One Patient P. 1)

	X	Y	Z	Psychiatrist
X	—	0.20	0.31	0.13
Y		—	0.70	0.59
Z			—	0.46
Psychiatrist				—

TABLE 3

	X	Y	Z	Psychiatrist
X	—	0.26	0.26	0 18
Y		—	0 71	0.52
Z			—	0.52
Psychiatrist				—

better fit might be made if two orthogonal factors are assumed, the second one having a small loading on X and Z. In this case the loadings could be as in Table 4. The correlations produced by these loadings are now given in Table 5. This is a better "fit," i.e., closer to the observed values of Table 2. However, we need not seek to extract every ounce of data, so to speak, from our data. If these happened to be the only data available, then no doubt we would work with the *two*-factor solution. But if we were to study many such sets of data, it might be sufficient to accept the result for one factor only and to deal with a second factor only if it exceeded

certain fiducial limits that we may set. In short, we might be prepared to deal with the more obvious results first, as in experimental work the world over. But the experimentalists' acumen and judgment are at issue. The acceptance of the second factor for Table 2 could assume importance if it appeared in due course that X and Z frequently gave such a result as this for different patients.

Factor I would be regarded as evidence for agreement between psychologists and psychiatrists, to the degree indicated by the loadings, *with respect to the factor*. We do not merely say that there is

TABLE 4

	Loading	
	Factor I	Factor II
X	0.25	0.33
Y	0.88	0.00
Z	0.80	0.33
Psychiatrist	0.64	0.00

TABLE 5

	X	Y	Z	Psychiatrist
X	—	0.22	0.31	0.16
Y	....	—	0.72	0.56
Z	....	....	—	0.51
Psychiatrist	....	....	....	—

agreement, but we say with respect to *what*. In this case it is with respect to factor I. Likewise for factor II if this is accepted: in this case only X and Z would show agreement with respect to this factor. That it is very modest in amount is indicated by the (hypothetical) correlation between these two psychologists for this factor—an amount $0.33 \times 0.33 = 0.12$, the probable error for which is of the order of 0.05. The correlation is thus scarcely significant or acceptable by customary standards, as we supposed.

By a factor such as I we mean merely another *hypothetical* Q-sort[2]

2. The factor-array has the interesting property that it is always an *operational* possibility, that is, a person can be found, in principle, who will provide it as his Q-sort. A person loaded 0.99 for the factor is such a case.

with the same 160 statements, of such a nature that X, Y, Z, and the psychiatrist correlate with it by amounts 0.30, 0.84, 0.84, and 0.62, respectively (for the first solution). We can make a best weighted estimate (see p. 175) of this Q-sort by determining the weighted *factor scores* of the individual statements. Thus the hypothetical Q-sort can be placed before us concretely. It may be regarded as what the case is *like*, as grasped by all four experts in their different degrees. We can seek to interpret it, i.e., to give it "meaning" in relation to the psychiatric formulations. Similarly for factor II, which will be another hypothetical Q-sort, uncorrelated with I, such that the Q-sorts about P. 1 by X and Z correlate 0.33 with it. It, too, can be placed before us concretely and offered an interpretation in relation to the psychiatric formulations. Clearly, means exist, along these lines, for determining what agreement is reached by the different assessors. It is as interesting to determine in what they *disagree*, and comparable methods are available for this, not only in terms of *communality* factors, but also by making use of *specificities*. The more pertinent the theory involved in the psychiatric formulations, in the above example, the more likely is the interpretation of factors such as I and II to be meaningful.

Such are the bare beginnings. We could continue in many directions. If Q-sorts are given under the above conditions for different patients $P_1, P_2, \ldots, P_N$, those about which the psychologists and psychiatrists are in agreement can be represented by their *factors* (indicating the agreement attained), correlated for the $N \times N$ patients, and these in turn factored with the object of determining what definable *types* of cases may be acceptable. Thus Beck has been able to identify such operationally defined categories of schizophrenic cases, which he calls $S_1, S_2, S_3, \ldots$, etc.

FURTHER METHODOLOGICAL ISSUES

The foregoing studies are for operations with the psychiatric formulations. The Rorschach indicators themselves are not used for Q-sorting. But as the formulations and indicators are paired, it might seem that the two are interchangeable. However, we have learned to be careful about translating data which are not tied directly to operations—it is the 160 psychiatric statements which comprise the sample and whose items are scored, and explanations should refer to these. We should observe that the psychiatric

formulations refer to one body of theory, and the Rorschach correlates to another. Some consideration of each is required.

In order to understand psychotic behavior, the psychiatrist nowadays thinks of it in dynamic terms; and, as we have seen, the *formulation* of Beck's study involved four main categories—*defenses*, *ego-condition*, *emotional state*, and *reconstitutive* forces. These can be represented formally as a Fisherian balanced block design (Table 6). The various "levels" are those mentioned or implied in the formulations provided by the psychiatrists under the headings *A*, *B*, *C*, and *D*. There are 3 × 3 × 3 × 2 = 54 possible combinations of these levels, one at a time for each main effect, namely, of the following order:

TABLE 6

Psychiatric Formulations as a Balanced Block Design

Independencies or Effects	Levels	No.	D.F.
A, defenses	*a*) Adjusted *b*) Self-preoccupied *c*) None in operation	3	2
B, ego condition	*d*) Regressed *e*) Inflexible *f*) Compensated	3	2
C, emotional state	*g*) Strong effects *h*) Unstable *i*) Regressed	3	2
D, reconstitution	*j*) Some *k*) None	2	1

a a	*a a*	*a a*	
d d	*d d*	*d d*	
g g	*h h*	*i i*	
j k	*j k*	*j k*	etc.

It is of some interest to note which of these should be found for schizophrenic cases. The combinations *cdij* and *cdik* appear to represent the regressed, hebephrenic case. Combination *begk* could be the schizophrenic child, still able to make some adjustment. Combination *afhj* might be the "ambulatory" case, who can become impulsive, unstable, and who is "holding onto reality" somewhat tenuously. But combinations such as *adij* are scarcely possible in fact. Along these lines it can be argued that only 10 or 11 of the 54 possibilities can occur, and if effect *D* is regarded as of lesser interest, only 4 or 5 remain as practical possibilities, namely: *afhj*, *begj*, *cdij*, and maybe *bfgj* and *bfhj*. (In each case *j* could be replaced by *k* without undue concern.)

Thus we may argue that the psychiatric formulations are likely to involve, almost inevitably, only 4 or 5 types of cases. The psychiatrist, of course, is not aware of these deductions, which we can put to test. Beck, in fact, discovers only a few factors which lead to such a limited number of *types*, now operationally defined.

However, what may be intended in such studies may not be what the operations discover for us. The psychiatrists are no doubt genuine in asserting this theory. If we wished to do so, it is possible to deal with Table 6 in a dependent manner. The *ad hoc* set of 160 statements used by Beck can be replaced by others, very like them but now covering the design of Table 6. Fifty-four formulations can be selected which cover the design once over, and for 3 replications a structured sample would result, size 162. Studies of the kind undertaken by Beck would then be for this sample, and the various *effects* can be tested by way of F-tests, for variance analysis and small-sample theory. Factors would be analyzed in this way, too, so that a dependent form of analysis would be pursued, to show what the psychiatric theory provides when empirical tests for it are made. All this has methodological advantages.

However, we may not be so certain about the premises, i.e., the postulated statements themselves. We like to keep our eyes open for other possibilities. Thus when, as in the case of Beck's studies, three factors (say A, B, C) are found, it is possible to identify them in relation to the psychiatric theory of the sample of statements with respect to which the factors are operationally defined. But we can also see what *other* possible explanations may be offered. To this end we believe that the aim of the researcher should be to determine whether the factors can be represented in some *other* factorial design as well. Thus Nunnally has suggested that three factors A, B, and C for three *types* of schizophrenia in Beck's study have the explanation shown in Table 7. The factors A, B, and C concerned *bce*, *bcf*, and *ace*. That is, these rather simpler conditions are all that appear to be at issue for the operations. They suggest that the *immediate dynamics* of the cases are grasped and not any matters of a developmental kind. That is, without knowing it, the testers and the psychiatrists alike appear to respond to the cases *as they are*, not with respect to their genesis, origins, or development. It is along such lines that, we believe, interesting conclusions can be reached.

RORSCHACH THEORY

Our conclusions about the Rorschach, in its methodological aspects, are very briefly reported below. They are supported by, or in turn support, the more general principles to which attention has already been given, e.g., with reference to the "single case."

The concern, needless to say, is with responses (or "indicators," as we have called them, since the location, determinant, and other bases of scores are analytical aspects of total responses), their *correlates*, and the *theory* relating these. The correlates are references to *behavior*—the example already given is perhaps worth repeating here: "$M! > C$" is an indicator, in our terminology, and "$M! > C \supset$ autistic fantasy" a correlate. Similarly, "edging" is an indicator, and "edging ⊃ odd behavior" a correlate. There may be many cor-

TABLE 7

REFORMULATION OF A SAMPLE

Independencies	Levels		No.	D.F.
A, ego restraint	*a*) Constriction	*b*) No restraint	2	1
B, thinking behavior	*c*) Disrupted	*d*) Coherent	2	1
C, fantasy	*e*) Autistic	*f*) Absent	2	1

relates for any one indicator, and, of course, there are hundreds of indicators and correlates. Ordinarily, different correlates would be at hand for specific purposes—thus those for the psychiatric formulations above are more particularly related to schizophrenic behavior, and a different set would be available for normal healthy persons. The various experts may also differ considerably in their *theory*: this will not concern us, but what is meant by a theory in this context will.

For reasons already given, we believe it best represents the scientific situation (92) for the Rorschach, to grant all correlates the status of facts, postulates, or premises. They are not just *inferences*, of the kind that "odd behavior" is a deduction made from "edging," or that "autistic fantasy" *should* occur for condition $M! > C$. Instead, they are to be regarded as previously tested propositions, which have been accepted (pro tem, and according to the fundamental principle of "permanent control" [92]) into the body of X's

(say Dr. Beck's) Rorschach doctrine. None is, or implies, any general proposition; none has any "general implications" (166), if by this is meant a function for isolated variables and individual differences. Each, instead, is relative to certain conditions, which we may refer to generally as those concerning personality-as-a-whole. None requires any direct proof, any more than one would seek to prove that a fly flies. Nor do *norms* mediate for them in any operational sense.[3]

It may be supposed that the correlates have been collected in relation to some body of theory. Rorschach (132) himself undoubtedly first did something of this kind. With a theory in mind, he was apperceptive for such facts, which he incorporated into his test theory. With a different theory in mind, he might have observed other correlates. The total number of correlates is not a fixed quantity, but ever increases.

TABLE 8

Independencies	Levels	No.	D.F.
X, control	*a*) Outer *b*) Inner *c*) Repressive (constriction)	3	2
Y, adjustment	*d*) Systematized anxiety *e*) Unsystematized anxiety *f*) Balanced	3	2
Z, Erlebnistype	*g*) Introverted *h*) Extroverted	2	1

The scientist, however, gains much if he can represent the main effects of a theory in a formal, usually a mathematical, model. In this way he may subsume many facts and may deduce consequences which have been overlooked. With this in view, therefore, we represent theories as balanced block designs. For Rorschach's own theory the design may be as in Table 8. Other Rorschach experts may have rather different main effects.

For Table 8 there are $3 \times 3 \times 2 = 18$ combinations of the effects, one level at a time. Since this theory would be basic to all else, all the Rorschach indicators and correlates must necessarily fit into it, in one or another of these 18 possible combinations. Thus $MI > C$ could refer to *cdg*. Sets of indicators or correlates can be

3. There is a proper place, no doubt, for *norms* in defining the bare Rorschach *indicators*. We doubt, however, whether these need to be other than rough-and-ready, or could be sanctioned as other than very approximate data. The fact that a person gives only one *M* response, for example, may mean much more in its correlate respects than 10 such responses may mean when given by another person.

compiled so as to cover the design completely, and this may be replicated at will. For z replications the set or sample would be $18z$ in size.

Thus, if it did nothing else, the design would be an aid to theory construction for the Rorschach, since it would compel attention to all possible indicators, in relation to the theory, in balanced design. It might even draw attention to indicators overlooked up to now.

What we said about the design for the psychiatric formulations applies no less here. These designs are not *tests* of the kind used in R-methodology, which have to be proved for their "general implications." It is in no way postulatory to such a design that a correlate, say "$M! > C \supset$ autistic fantasy," be placed in cell *cdg* of the design because "on the average" it so occurs in the general population. Whether it does or does not so occur is a matter of fact which has no relevancy to our use of the test.

Nor does the Rorschach tester use such facts when he makes his analysis of a case. He pursues his analysis, instead, as a sequence of arguments, in which hypotheses about the case or personality are grasped, these, in turn, being supported elsewhere among the test indicators. It is a closely reasoned affair (Stein, 154), only the conclusions to which can be put to any empirical test; and the expert may employ this or that correlate for a particular indicator—no rule-of-thumb matters are at issue. He pulls the correlates out of the body of doctrine, so to speak, as he wants them. In this manner he produces a personality description, or a problem about a case, or a diagnosis. These descriptions, however, as we have seen, lack all operational definition.

We have two fundamental principles, therefore, to contend with. First, no correlate can be taken out of its context—this is the premise of personality-as-a-whole and of the impossibility of testing correlates as general propositions with respect to individual differences. Second, the tester's skill is a complex use of Rorschach theory, i.e., such as draws upon all previously accepted propositions or correlates as required, in a "theoretical" manner, i.e., by reasoning about correlates.[4] The difficulty in the past has been to provide *operations* in agreement with these principles, from which their scientific study can follow in the inevitable way of science.

4. We would regard so-called "intuition," methodologically, as skill or implied reasoning.

It is our thesis that Q-technique satisfies the first principle and approximates to the second in a surprisingly satisfactory degree. Given a set or sample of correlates (under conditions that the sample is truly representative of the whole body of correlates—a matter that is admirably attended to in a structured sample), each correlate is used in relation to all others of the sample. None is ever taken out of this context, which is a description of some aspect of personality-as-a-whole, provided under specific conditions of instruction by the Q-sort. The relative position of a correlate in the sample can then operationally define its "significance" under given conditions, which include personality-as-a-whole. By "significance" we mean what J. M. Keynes (94) meant by it in correlational theory—it provides a basis for the all-important transitory postulate, without which we could scarcely study any order or uniformities in behavior. These are the conditions, clearly, which also mediate in Q-methodology.

Similarly with respect to the *act* of making a Q-sort. This ideally involves the same arguments and the same reasoning that go into the usual analysis of the Rorschach. The only limitations are that some indicators and correlates which the analyst may have wished to use may not be available to him. But he has compensating discriminations available to him, in all the comparisons and cross-references for the many correlates of the Q-sample. The concept of "significance" is also perhaps more helpful than the Rorschach analyst imagines. And, unlike a personality description or a qualitative conclusion, the Q-sorting ends with a quantitative conclusion. We might even suppose that the arguments during a Q-sort are so diversely ramified among the items of the sample that the relative contributions to the quantitative conclusion are indeed well represented by a normal distribution of scores.

Such, then, are some of the principles attending the Rorschach. The applications of Q-technique to the situation are not arbitrary or "means-centered" tricks of a statistician; they are precisely such as the Rorschach principles themselves demand for proper satisfaction of their conditions.

Similar principles apply to all the projective tests. There are some points of interest for the *Thematic Apperception Tests*, however, and we now address ourselves to these.

THE THEMATIC APPERCEPTION TEST

The TAT has interesting roots in history and presents altogether fascinating behavioral possibilities. It may seem a long way from Comenius, the great Czech educator of the sixteenth century, to Binet, the French psychologist of fifty years ago, and even further from these to Murray (117) and his co-workers at Harvard. Psychologically, however, the distance is not very great, for all alike were fascinated by the thematic possibilities of illustrations, the pictures in story-books, especially those depicting human beings in interaction. The old Latin primers in their simple way, and countless school books since, have for centuries invited children to tell a story about a picture. Binet (25) elevated the storytelling to the status of a test of "report." Spearman (149) and others employed the test in the fashion of the time, only a few decades ago, to examine the "psychological relations" involved in, or elicited by, pictures. It is now called the *Thematic Apperception Test*. Stories are still told about pictures, but they are now interpreted, as dreams are. Murray, in particular, looks for main themes or *themas* in a person's stories, and these, it seems, reveal the personality of the storyteller, his emotions, sentiments, conflicts, and the like. Murray says:

> Special value resides in its power to expose the underlying inhibited tendencies which the subject, or patient (*the story-teller*), is not willing to admit, or can not admit because he is unconscious of them [118].

As for the Rorschach, "objective" psychologists consider that the TAT lacks objectivity. There are no *norms* for it, and not even a standard set of pictures for orderly presentation under standardized conditions—we may choose any pictures we like. It issues in a lot of verbal report, peculiar to each person, which, of course, is suspect by those who think of this as anecdotal and idiosyncratic. Yet, for our part, the lack of *norms* is the test's essential virtue. The difficulty about the test is not its supposed subjectivity; instead, neither its critics nor its practitioners have discovered how to study the test in its own terms. Theories about it are likely to be general theories of personality and not, on the whole, theories or hypotheses grounded in the test's own intrinsic possibilities. A method is required which can deal with the test from the standpoint of the "essential centrality" of the individual, without regard, therefore,

to *norms* or the like, but clearly and concisely in terms of propositions arising out of the test itself, for their proof or disproof.

BASIC FORMULATIONS

The TAT has become a sort of sounding box for picking up and amplifying almost anyone's particular theory of personality. Murray, almost alone, has offered a theoretical framework peculiar to the test's inherent possibilities. He proposed that the TAT should be analyzed in terms of the themas which run through a sequence of pictures like a theme in music. Such a thesis is easily tested experimentally. We would regard it as specific to the test, therefore, to determine how far these supposed themas can be identified by different interpreters, whether the subject can come to ready insight about them; whether mature persons, free from "presses," ever have a thema; and whether themas have any *necessary* connections with the personality as such of their spinners. For if a woman loses her most precious possession, might she not be preoccupied by the loss, project it everywhere, and do so without giving away anything about her personality, except perhaps solicitousness? In any case we would like to experiment with questions of this kind rather than regard the test as a "give-away" of symptoms, personality traits, and the like structural properties of personality.

As applied by Murray, the subject is encouraged to tell a story about each of several pictures, inventing each on the spur of the moment and saying what is happening, what led up to it, and what the outcome might be. It has never been our own practice, however, to encourage the notion that what one wants essentially is a story. The picture represents, initially, a relatively unstructured situation in which the subject is placed, and we like to keep it so by asking the subject to "tell about it." The spontaneous reactions of the subject are reached in this way. Subsequently, when some "report" has been provided, it is insinuated (rather than given as a formal instruction) that the information might be put together in the form of a suggestion that "such-and-such is happening . . . because of such-and-such . . . and that the outcome is likely to be . . . such-and-such." That is, we try to steer away from orderly or constructive "thinking about" the picture and to approach, instead, a freely associating situation similar to that reached in psychoanalysis. The

test, therefore, is not presented as a "test of imagination." Murray, however, applies the test differently. He has to say, for example:

> If the pictures are presented as a test of imagination, the subject's interest, together with his need for approval, can be so involved in the task that he forgets his sensitive self and the necessity of defending it against the probings of the examiner, and, before he knows it, he has said things about an invented character that apply to himself, things which he would have been reluctant to confess in response to a direct question. As a rule the subject leaves the test happily unaware that he has presented the psychologist with what amounts to an X-ray picture of his inner self [118, p. 4].

It seems that different standpoints are here at issue. Murray could proceed along the lines of "testing the imagination," and we with an orientation toward "spontaneous associations," with little but opinion to support either. No doubt wide experience could be invoked, in due course, in support of one or the other, but it would be interesting to put such matters to direct test, at once, in a few test cases. This can be done along Q-lines, a matter that we leave as an exercise in experimental methodology for the reader himself. We have other experiments in mind for the moment.

But, to continue, those who search for norms about a person's "inner self" or his "sensitive self" or any of the other defenses and the like with which Murray is concerned have a difficult task ahead. Yet Murray is quite correct in supposing that such matters are involved, although, up to now, no one has been able to test them in any compelling and direct manner. Experiments, of course, have been conducted, which gather data of an incidental or "objective" nature for the main part: Sanford (135), for example, found that fantasies about food increased in the TAT reports of men on a starvation diet. It is another matter to operate with only *one* person's protocols and to experiment with his "sensitive" and "inner selves" as we can now do in Q-technique terms.

It is clear that the test may involve *projection:* it is, indeed, the best-known of the projective techniques. But not all interactions with the TAT pictures should be regarded as projections. The pictures, after all, depict something more or less *real*, representing concrete interactions between persons, without stretching anyone's imagination. We find little, if any, reference to this possibility in current literature on the TAT. Having called the test "projective,"

then, willy-nilly, everything about it is regarded as obviously structurally or functionally projective; the truth is that well-developed or well-defended personalities either do not project in the functional sense or else have no need to do so.

We should confess here to one study with the TAT which is apposite to the matter just raised. It is well known that British women smoke a lot of cigarettes, and we were involved a year or two ago in a study of their smoking habits. We soon found it useful to divide women smokers into two categories: (i) those who smoked heavily but without compulsion and (ii) those who smoked heavily and confessed that they "couldn't give it up." The former enjoyed smoking on the whole, at best as a connoisseur loves his wines. The latter (ii) *needed* the smoking in a deeply upsetting fashion. The former *could* give it up if required; the latter felt that they could not possibly do so. A set of TAT pictures was applied to a number of women classified previously as either i or ii, i.e., what might be called *balanced* as distinct from compulsive or *need-situation* smokers. The results were clean-cut. In every case the women of category ii projected functionally; in no case did the women of class i do so. The latter responded to the pictures in a "natural," "reasonable," "consistent" manner, describing sensibly what *could* be at issue, without direct personal involvement. It was not that they were not being "imaginative"; on the contrary, their storytelling was creative enough. They were well adjusted and in no need of themas to relieve them. No doubt this mirrors the composure of these persons, and it is not the same thing, as we have said, as functional projection. Rather, the responses are *sublimations*, evincing free interaction between the women and the pictures, an example of true *transaction* (see note to p. 24, chap. i). But the projections of the women of class ii were of a very different nature. In each case some obvious "drives, emotions, sentiments, complexes and conflicts" of the individual were clearly evinced. The matter was tested very roughly as follows: After testing a woman, the results were discussed openly with her, in such a way as to probe into any projections. In the case of women of category ii it was a very simple matter to open up in this way all sorts of stresses that they were currently undergoing, from serious troubles with husbands, or others' husbands, to financial worries, guilts about Lesbian exploits, and other, even murderous, endeavors. But, with respect to class i, matters were

very different. Nothing that we could do, however subtle the approach, could disentangle any such conflict situations. They may have been able to hide them better, but we doubt it, for it was not that, in some cases, they were without their worldly or real troubles; one had lost her husband a year before, but had faced up to a new life and was now adjusted, in spite of a deep loss. Another was enjoying an illicit love affair and was upset only when no such opportunities existed for her. Others were active businesswomen, with the usual stresses of such a life. But all were adjusted, composed, in command of themselves, as the best Englishwomen can be.[5]

These, then, are some of the matters more uniquely at issue in the TAT. The distinction between sublimations and projections seems to have been overlooked by the experts; it is of essential interest, however, to ask whether the two mechanisms can be distinguished in Q-technique terms.

The key to any testing of such matters, however, must lie in the frank acceptance of the TAT as a situation involving the interpreter as well as the protocol. The subject, test pictures, and tester or interpreter and the effective conditions under which the testing takes place are all elements in a complex behavioral setting. We do not know, for example, how far the lush setting of a luxurious Berkeley Square (London) sitting-room, in which the TAT tests were applied to the men and women smokers in the above study, entered into the interactions: the soft carpets, suggestive of gracious living, and the expensive furniture might well have been as effective an influence as the TAT pictures themselves. But these are matters for proof or disproof; meanwhile, the more usual settings for our experiments are likely to be the bleak, inartistic, forbidding benches of a university office.

But the TAT has its main use, apparently, in elucidating personality as such. In the hands of a psychologist with a sound theory, the test seems to "work," and to work wonders, probing into the

5. Men smokers, we thought, would perhaps not be so likely to give themselves away. But the results were the same for men as for women. Where the man confesses that he is a "slave" to the habit or that he "knows he smokes far too much, but couldn't give it up," then projection of stresses was immediate, convincing, and very surprising. But where the smoker was under no compunction about his smoking and agreed that he "could give it up if he wanted to," no functional projections occurred, and no probings by us ever unearthed the raw emotions that were so characteristic of the slavish smokers.

dynamics of personality. Skill at interpretation, however, is likely to be similar to skills in general—it is not easy to discover the syntheses they involve. But it is now a straightforward matter to represent any personality theory in a "sample" of statements for Q-technique purposes, and therefore, theoretically, it should be possible to put the TAT and its interpreters to test in terms of any theory or theories presumed to be at issue.

With the foregoing background, then, we are ready for experimental work. It has been our purpose to touch upon a few particular matters, such as those of functional projection versus sublimation and "imagination" versus "free association," to indicate inherent matters at issue. Research upon them is necessary, and it is a simple matter to design the appropriate experiments along Q-technique lines.

PROTOCOL ANALYSIS

It is impossible to report here our studies with the TAT in any detail. One or two have methodological interest. In one we raised the general question as to whether the TAT "stories" or protocols could themselves be the basis of samples of statements for Q-technique purposes, and in another whether TAT experts made use of Murray's theory in the act of interpretation. In others we showed under what conditions the experts were able to diagnose personality for different Q-samples of statements.

In the first-mentioned we arranged, with Mr. J. C. Nunnally, to take a complete record of a subject's responses to 16 TAT pictures of the Murray series (118). Typed copies were made available. The subject was Rogerg, mentioned earlier (p. 255). The "stories" consist, of course, of sentences. It is possible to consider these as a "universe" of statements which can be sampled for Q-technique purposes. Given a sample of, say, 100 sentences or parts of sentences from Rogerg's data, it is not difficult to suppose that they may be quantified for their relative *significance* or importance in the general psychological context provided by the "stories" or for their importance as indicators of the storyteller's personality or with respect to some other particular theoretical issues. The drawing of samples of statements from protocols and their subsequent factor analysis have wide application. Two kinds of samples may be taken:

a) One might be chosen at random from the protocols. Any 100 might be chosen, indicating them in the contexts by *underlinings* or the like. The sample could then be judged for the significance of each sentence relative to the others, *in the total context of the "stories" as such.* The appraisal could be repeated for any other sample similarly chosen.

b) It would be better, however, to draw up a sample of such statements which represents a hypothesis or proposition. In the present case, for example, we might choose a structured sample which involves Dr. Murray's thesis about a thema, so that it can be put to test.

We pursued the latter course (*b*). As our hypothesis to be tested, we represented a thema which, in our view, was contained in Rogerg's protocols. It concerned a certain "regression to childhood" theme, recurrent in the stories. If experts pursue Murray's instructions, the thema is likely to be apparent, and the statements involving it crucially are likely to be regarded as highly significant ones. If experts work along different lines, e.g., with attention to *structural* indicators, then they are unlikely to give the same significance to statements which support the thema. Murray's thesis was thus represented as a single independency at two levels. Half of the sample consisted of statements bearing directly on the thema; the other half did not do so, and yet looked very like the former in superficial ways, statement for statement. More complicated designs, of course, could be composed if required.

The statements, 120 in all, were underlined in the body of Rogerg's protocols and were duly numbered. Certain steps were taken to assist the operators in the practical matter of making their Q-sorts with these statements.

The experimental design involved 8 TAT experts and 8 novices. All were set the task of appraising the protocols blindly, with no knowledge of Rogerg other than what his "stories" could reveal. In addition, Rogerg himself was invited to give his own appraisal of the statements, as was the TAT expert who applied the tests to Rogerg. The experimenter, Stephenson, also gave his appraisal. In all cases the instruction was to array the statements in order of their "significance" for understanding Rogerg's personality.

Altogether, then, 19 operators were involved. Variety was added to the design by allowing 4 of the experts to *hear* the recording of the testing prior to, or contemporaneously with, study of the printed protocols. Likewise, 4 of the novices heard the recordings. The complete experimental design is shown in Table 9.

The design covers a number of propositions, then, which factor analysis will test. We do not state them in a formal fashion but prefer to have them listed as follows:

1. The experimenter (E) represented the thema: he knew what the thema was, and arrayed the 160 statements in accordance with it.[6]
2. The experts would correlate with E's array if they made use of the thema.
3. If the experts did not grasp the thema, they might still agree among themselves, but for reasons other than those concerning the thema.
4. The novices would not grasp the thema but might show agreement among themselves, on common-sense grounds.
5. Hearing Rogerg's voice should make a difference and be represented as a factor for those concerned, whether experts or novices.
6. Face-to-face acquaintanceship with Rogerg, as for the tester and the experimenter E, may also influence the arrays.
7. One might wonder whether Rogerg's self-awareness is comparable with the common-sense inferences of the novices.

TABLE 9

SAMPLE: n = 120 STATEMENTS; SUBJECT: ROGERG TESTED BY TAT

	Protocols with Recordings	Protocols Alone	Protocols and Knowledge of Rogerg
Experts	No. 2 No. 3 No. 4 No. 5	No. 6 No. 7 No. 8 No. 9	No. 1
Experimenter			No. 10
Novices	No. 11 No. 12 No. 13 No. 14	No. 15 No. 16 No. 17 No. 18	
Rogerg			No. 19

Table 10 gives the correlations, and three factors are adequate for it, as follows:

I′ is common to experts and novices alike, but is not much in evidence for the experimenter (No. 10).

II′ is *positive* for all experts and *zero* for the novices.

III′ is *negative* for some of the novices.

6. It would have been better to explain the thema to some three other expert TAT interpreters and to get them to sort out the cards as well, so as to represent the proposition by four variates. But their correlations with the experimenter's would undoubtedly have been very high.

TABLE 10

CORRELATION COEFFICIENTS FOR TAT PROTOCOL STUDY (STEPHENSON-NUNNALLY)
(n = 120 Statements)

	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	Centroid Factors I′	Centroid Factors II′	Centroid Factors III′	Rotated Factors (Orthogonal) I′	Rotated Factors (Orthogonal) II′	Rotated Factors (Orthogonal) III′	"Types"
1	—																			53	−27	−13	51	02	−24	*A*
2	30	—																		57	22	16	45	43	−05	*AB*
3	23	39	—																	60	13	17	59	40	−07	*AB*
4	−10	23	30	—																22	36	24	07	42	10	*B*
5	29	33	49	20	—															61	18	−15	42	35	−35	*A*
6	21	31	28	30	26	—														42	30	03	23	43	−15	*B*
7	07	29	24	16	26	27	—													41	30	−20	16	40	−37	*B*
8	24	35	27	12	21	29	18	—												47	18	−10	32	32	−27	
9	27	31	15	10	29	12	29	17	—											40	14	−21	25	20	−34	
10	24	11	27	−08	21	10	12	10	18	—										37	−15	−10	36	−01	−16	
11	31	28	30	15	31	21	28	31	13	24	—									59	−15	−03	60	10	−17	*A*
12	22	25	28	05	09	−01	09	11	03	32	19	—								36	−31	17	51	−06	10	*A*
13	29	12	20	01	09	−01	−05	−02	14	17	33	31	—							30	−43	11	50	−21	09	*A*
14	35	36	38	08	35	21	07	33	19	36	35	42	20	—						63	−28	13	09	06	−01	*A*
15	29	08	10	−12	37	10	02	11	22	06	31	−02	18	25	—					38	−20	−50	34	−17	−54	−*C*
16	29	25	33	−01	49	19	29	20	22	04	23	−09	−08	20	30	—				48	20	−47	26	20	−61	−*C*
17	39	29	22	−01	23	21	36	17	15	06	30	04	19	34	36	35	—			51	−13	−27	46	01	−38	*A* (−*C*)
18	28	19	32	15	32	15	14	28	09	08	36	27	19	45	24	23	33	—		53	−19	−07	54	03	−19	*A*
19	14	19	05	05	14	20	18	36	19	31	26	17	−11	21	13	16	13	15	—	37	08	−38	22	05	−46	−*C*

The "typification" is shown at the right of Table 10, the results being as follows:

A is pure for No. 1 and for novices 11, 12, 13, 14, and 18.
B is pure for experts 4, 6, and 7.
C is pure for novices 15, 16, and Rogerg (19).

The conclusions are:

a) Factor I′ must be of a common-sense nature, since it is common to experts and novices alike. It in no way "gets at" the thema.
b) Factors II′ and III′ differentiate experts from novices, but the thema is not involved, since there is no loading for No. 10 on the factor.
c) Hearing the recordings has no apparent or consistent effect.
d) It is clear that the experts did not deal with the thema, and face-to-face contact with Rogerg did not affect the results consistently for Nos. 1, 10, and 19.

Of course, we could have asked the experts *how* they analyze such protocols; but we wanted an example, and, in any case, how experts operate is likely to be rather different from what they sometimes suppose. Moreover, we can now place before us the precise factor-arrays for I′, II′, III′ or for the "pure" types *A*, *B*, and *C*, or their mixtures *AB* and *A*(−*C*), and see in detail what they involve.

The factor-array for III′ (type −*C*) cannot be given here, but its nature can be gathered, perhaps, from the statements which were regarded as the most significant of all by the novices. They are the following:

Red*

No.
12 . . . . "They should take the girl and the woman out and just leave the man there plowing the landscape."
16 . . . . "I don't like the people's faces in this at all."
26 . . . . "There is a sort of guilty connotation."
11 . . . . "She looks rather resentful and disturbed."

Blue*

6 . . . . "There is an awful looking woman standing under a tree."
26 . . . . "This woman has an awful face."
18 . . . . "Funny looking people."
30 . . . . "She looks a little bit taken aback, or maybe she is just looking at him in a quizzical way—you know, the way some people do to be sarcastic."

* These terms have reference to the fact that the 160 statements were dealt with in two samples of 80 each, one called "red," the other "blue."

With the exception of No. 12 (Red), all these statements refer to painful, ugly, or displeasing emotions—even the word "funny" is not a reference to comicalness but to suspicion and the like. But the TAT pictures at issue really *do* look a bit like this, in these particulars; the novices, therefore, have merely picked upon these obvious emotional states. But they are regarded as the really significant matters, telling most, not about the reality situations in the pictures, but about Rogerg himself. Confluence of this kind is a widespread source of human misunderstanding, and our novices can scarcely be blamed for making such an obvious mistake here. None of them, apparently, argued that it might be more pertinent to look for statements of an unusual, irrational, or unrealistic nature, i.e., with little real bearing on the TAT pictures as such.

The factor, therefore, represents such a confluence: extreme emotional states wherever referred to are likely to be regarded as significant in personality, in this case in Rogerg's personality. Needless to say, it can have little validity, as such, vis-à-vis Rogerg.

Factor II′ (type *B*) provided a factor-array with the following statements most "significant":

RED

No.
54.......“Maybe his hands are tied together, and he's just wandering about that way.”
12.......“They should take the girl and the woman out and just leave the man there plowing the landscape.”
59.......“It looks as if one eye is looking at the girl's face and the other eye is looking away.”
55.......“They might be eyes.”

BLUE

35.......“The face of the man who is dying [*sic*] down.”
60.......“She might have her nails stuck into the other person's neck.”
11.......“He's like a part of the scenery, you know.”

These involve remote inferences about motives (Nos. 54, 59, 60), symbolization (Nos. 55, 11), and the importance of slips of the tongue as indicative of unconscious tendencies (No. 35). In short, the factor is the work of experts who have "psychologized" about the data in a technical fashion.

Factor I′ (Type *A*) was essentially a matter of common sense, without the mistake made in factor III′ of identifying an ugly or emotional feature in the protocol statements with Rogerg's presumed personality and without making the opposite mistake of "psychologizing" about the case, which seems to have happened for factor II′.

Our example, then, has brought us a long way methodologically. Sufficient has been accomplished to indicate what is involved in the analysis of protocol, with regard to *sentences*. The factor results are very simple; yet they bear upon every one of our propositions, at the first rotation to simplest structure. The designing of such experiments, involving "control" subjects, and the direct *representing* of propositions (such as concerns No. 10 above, for the Murray doctrine of *thematic* apperception) are outstanding features of Q-technique.

The study itself is only one of many that need to be pursued about the intrinsic issues for the TAT. Others, concerning projection versus sublimation and the like, are not difficult to design but must be left for another occasion.

ASCRIPTION OF PERSONALITY FROM TAT PROTOCOLS

We now turn, finally, to the TAT and its role in the study of personality. The TAT is employed to discover motives, interests, and sentiments; in a word, the storyteller's personality. It is fairly certain that the TAT experts make use of a great deal in the protocol besides straightforward thematic information, and perhaps every expert has his own approach to the test in this respect. Dr. Morris Stein (154), for example, regards the TAT test situation as a "play within a play." The heroes in the patient's stories constitute one play, and the patient's approach to the TAT (and to the examiner) constitutes the other. These "behavioral situations" are analyzed by Stein precisely as he would analyze any social situation, that is, in terms of a theoretical frame of reference about personality in social interaction. The analysis consists of a step-by-step elucidation of the patient's personality; hypotheses are developed at one point and are clarified or rejected in terms of later evidence at other points. In short, the protocols become a complex testing ground, upon which Stein educes hypotheses and tests them *in situ*. Having arrived at

many hypotheses in this way, an attempt is made at the conclusion of the analysis to combine them all in a meaningful fashion, and this constitutes the clinical or personality description of the case. This, again, is, in effect, a set of propositions about the personality, such as "he appears frightened, depressed, and tense" or that "he perceives himself to be a child in many ways—helpless and in need of care and protection by others." But it remains a set of untested propositions.

Our own particular interest is the methodological one of offering some proof for such interpretations. We would like to see a few crucial experiments performed, to prove to our own satisfaction and to cautious scientists in general that all is well in this region of rich clinical regard. Thus where the interpretations are based upon a theory of personality, it should be possible to represent this "frame of reference" in a sample of statements, much as was done for Jung's theory, and to put the expert and his theory to test. We have conducted several experiments of this kind, all with Q-methodology at issue, reaching the conclusion that experts, under appropriate conditions, can achieve valid descriptions when they work "blindly" from TAT protocols.

CONCLUSION

The studies we have been considering are the first of very many that we can anticipate with confidence. Q-methodology makes them possible, where hitherto it would have seemed impossible to conduct them. We argue that it is methodologically best to regard the projective test instruments as, at best, instigators of problems about the single case, the concrete person. The singular situation is then studied in its own terms. A theory can be represented in designed Q-samples; this, in principle, removes the need for *norms* or formal scales of any kind and for the rooting of one's investigations in the exigencies and postulates of individual differences and general propositions. The problems themselves are intrinsically matters for variate designs, leading to empirical discoveries. Along such lines the clinical psychologist becomes an investigator rather than a diagnostician; and projection tests merely become steps in a longer chain of empirical inquiry about a personality.

CHAPTER XIV

APPLICATION TO CLINICAL PSYCHOLOGY

PURPOSE

The clinical psychologist has an almost unlimited range of possibilities for research open to him, since every patient he has is a source of possible experimental inquiries. These, of course, will take the investigator into much detail. The several studies already reported in which Q-technique has been employed, such as those of Hartley (76), Heine (79), Pemberton (123), Rogers and his staff (129), Fiedler (65), and Nunnally (120), are much too long for discussion in the present chapter, even with respect to their methodology only. Nor is it our purpose to examine the contributions that these and similar studies have made to any advance of clinical psychology in its experimental aspects. Our concern, instead, is to elaborate upon one or two methodological principles which are of importance in this area of study. Much, indeed, has already been referred to in earlier chapters which is of direct concern to clinical psychology. But there are a few special matters which deserve mention, all of a methodological nature, which can be given some attention most cogently, perhaps, in relation to clinical studies.

A few points of a general kind are involved. Clinical psychology has always been more particularly associated with the methodological problem of the "single case," which, it would seem, is now solved. It would be foolish to suppose that Q-methodology is the only solution, or the first one. But statistics and the use of large numbers are as closely linked as lawn and grass or seashore and sandy beach, and, in the past, it has been believed that in psychology, somehow, statistics *meant* that a large number of *persons* must be at issue. We now see clearly that statistical procedures can be brought to bear upon a single case, within its own confines. The effects of the old misconception, however, are still with us in many unsuspected places.

The use of small-sample doctrine is quite another matter. This

has more to do with the design of experiments than with statistics as such. A mistake will be made, however, if it is thought that, in recommending the use of small-sampling doctrine and single-case studies, this is at the expense of legitimate accuracy and precision of experimentation. Within the confines of a single case it is still essential to achieve statistical precision and power—a Q-sample of 150 cases or more is intrinsically better, other things being equal, than one of half this size. Estimate of error based on 100 degrees of freedom is a wiser security than one based on 10 only. We are sure, too, that two studies along comparable lines, pointing to the same end, performed on the same subject, are better than only one.

Our insistence on single-case studies, in the last analysis, has much to do with the obvious complexity of human behavior, such as is studied in clinical psychology. When propositions can be tested only in relation to large numbers of cases, rather simple matters are likely to be involved, altogether out of keeping, when one thinks of it for a moment, with the complex matters at issue. Both the propositions and their *instrumentation* may be crude. We are reminded in this connection of the editor of a scientific journal who wished to determine some operationally defined facts upon which to base his editorial policy. A list of papers was prepared, which had appeared in his journal the previous year. Under careful sampling conditions a large number of scientists were solicited and invited to indicate which of the papers they had read. Facts are no doubt gathered this way. But if one stops to ask how a particular subscriber *uses* the journal, the situation is not in the least covered by merely asking which papers had been read—the subscriber may look briefly at many papers and make a note to read some of them later; some he may work *at*, rather than read as one reads a newspaper; others are perhaps mentally put in place and are forgotten about until, under appropriate conditions, it is remembered that such-and-such a paper appeared in the journal. Such are a few of the concrete behaviors that are missed altogether in "instrumentation" which asks merely for a count of the papers actually read. Similarly in psychology, especially in clinical and personality studies, much well-meant experimentation is pursued with inadequate expectancies of complexity. We would like to deal with the matter, however briefly, in relation to clinical psychology. The reminder is again given that by "single case" we mean "single inter-

actional situation." If a person A is interviewed by a number of others, X, Y, Z, . . . , we wish to study this situation as such in its concreteness, and this is the "single case."

With these matters in mind, then, we can look briefly at clinical psychology from our Q-methodological standpoint. Basic research, we would argue, can best be undertaken in relation to the single clinical case.

CHANGES IN THE SELF AND SELF-IDEAL DURING THERAPY

Reference has already been made to Hartley's study (76) of a single case, in which the object was to investigate the changes occurring during therapy in the self-descriptions of the patient. These so-called "perceived" selves are central to the client-centered theory of personality (130), and the fact that an instrument is now available for bringing them to testable form is, one feels sure, a matter of considerable significance. Some idea of the matters at issue is offered, perhaps, by noting a few of the tentative conclusions reported in the University of Chicago Counseling Center's *Second Interim Report* of November, 1950. The concern, needless to say, is with client-centered therapy:

1. The "perceived self" and the "perceived self-ideal" become more congruent during client-centered therapy.
2. During therapy the change in the "perceived self" is much greater than the change in the "desired self."
3. Change in the self during a period of therapy is greater than during a period of no therapy, even when all other conditions are matched, including a matching of the desire for help and changes in one's self.
4. The self-ideal tends to become more unique, more idiosyncratic, during therapy.
5. During therapy the "perceived self" changes markedly, and the self at the conclusion of therapy often shows little resemblance to the self as it existed at the beginning of therapy.

There are other conclusions of the kind, tentative no doubt, but all of them alike with respect to the fact that discoveries are at issue. We assert that each should be studied, basically, in relation to the single case.

How this can be done, in principle, has been discussed already for the case of Rogerg (p. 255). Nunnally's study (120) is an example of the kind of complex interrelationships that can be encompassed

within the confines of a single case. The case was studied before, during, and after therapy, in terms of its own *self-assessments*, much as we described in chapter xi. It provides examples of the main themes of this book: *first*, what might have been regarded a few years ago as unacceptable to scientific regard, a factor study on a single case, is seen to be quite permissible; *second*, what would have been postulates in an informal hypothetico-deductive framework become general theoretical propositions; *third*, a single case is first represented by a limited number of variates, which, after factor analysis, are used as a basis for making predictions about the further course of the case; *fourth*, the predictions are put to test by way of dependency factor analysis: *fifth*, there is close reasoning about the course of the case, qualified at each step, however, by the empirical data; and *sixth*, interesting and not altogether expected results made their appearance. We would like to refer to the latter, in one detail, for its methodological interest. Reference was made, in the preface to Part II, to the need for some caution about the dependency methods in science. An overweening curiosity, we said, rather than reasoning within a formal hypothetico-deductive framework only, is to be welcomed and encouraged. An experiment, therefore, is much more than the fulfilment of predictions, or of the empirical testing of previously asserted propositions. It is an opportunity for making discoveries. These may be incidental or accidental, trivial or important. Fleming discovered penicillin rather accidentally, but the result was profound. In psychological research there should be room for matters of the kind, *as an intrinsic feature of the methodology*. Factor analysis, in particular, is peculiarly rich in this respect: it offers many possibilities for unexpected, but not altogether incidental, results. Any table of correlations for twenty or more variates, duly factored, can hold together many interrelated propositions, all of which must conform and fit together, like pieces in a jigsaw puzzle. Some of these can be dealt with in the usual manner, by way of the testing of predictions. But others come to light which could not have been predicted, and yet are more than afterthoughts. They are not mere matters of being wise after the event but are, instead, substantiated inductions that could not be other than what they are in relation to the other facts at issue.

Thus it was no part of Nunnally's formal design to make predic-

tions about what would happen to his single case in relation to two psychologists, A and B, who were involved. The case was first seen by A, who adopted a "supporting" attitude toward it. He tried to reassure the patient, to prevent any immediate exposure of the obvious conflicts at issue, pending treatment. The patient was then passed over to psychologist B for treatment. Meanwhile, the case made many self-assessments, as indicated in chapter xi, at the time it was supported by A and treated by B. The self-assessments were of two kinds, those regarded as "owned" *by* the case, and those that the case believed were "attributed" *to* it by others, including A and B. The results, prior to treatment, and brought to light very concisely and neatly in factor terms, show how remarkably the patient could not depart from the supportive influence of reassurance, affection, and friendly advice given by A. Self-assessments made at this time mirrored this composure and social aplomb. Under treatment, however, defenses crumbled away, and the case could permit B to attribute qualities to her of a totally different character. Yet, in doing so, the case was able to move ahead toward a more stable personality, evidence of which, again, was reached in factor terms.

It is perhaps only a small point of *fact*. It consists, however, of two inductions. *First*, along Q-lines a purely supportive interaction is shown to be distinguishable from one of therapeutic counseling. This is indicated by most obvious facts, that is, in terms of the case's own operations and their factor analysis. *Second*, these facts square with others in the general body of the factor data, which indicate that, as the case so changes, there is a corresponding change in the integrated aspects of its self-assessments—the personality becomes more stable and less dependent. We do not expect to repeat this set of findings (which are only a small part of the whole) in detail in other cases. But the difference between purely supportive handling of persons and their therapeutic treatment is now probably worth further attention, for theoretical reasons. For character, it has always been believed, and much of social sensibility and apparent decency are supposed to be fostered by way of affection and good example: the psychologist, however, may grasp something very different in the permissiveness and self-acceptance of counseling and therapy. Although it is true to say that incidental discoveries of this kind may emerge from almost any experimental study, whatever the

techniques may be, there is a certain richness of inductive opportunity in factor analysis; on this, indeed, has depended much of R-technique's success as an inductive method. In Q-methodology the possibilities are even greater than in R, because of immediate access to the samples used in Q and because of the ready way in which variates in Q can be related to one another.

As we said, it is not our purpose here to discuss the results obtained, as such, in clinical experiments of the kind undertaken by Rogers or Nunnally, except in so far as methodological matters are at issue. A recent study by Pemberton (123), in which Q-technique was used to some advantage, deserves mention on such grounds. It dealt with the diagnostic skill of clinical psychologists. Only one subject, X, was employed. But many different clinical tests were brought to bear upon him, with the object of studying the skill of the clinicians. From our methodological point of view the study is of interest because of the different forms of factor analysis to which it has been subjected. Pemberton factored his data along Thurstone lines, reaching simple structure, for correlated factors. We have refactored it along dependency lines, reaching a different, and yet not inconsistent solution to the data.

The subject X was tested by Rorschach, TAT, Sentence Completion, and Standard Test procedures (the latter involving the use of the Wechsler-Bellevue and other tests). Three clinical psychologists were allotted to each of these four tests, and independently made diagnoses of the case, blindly from the protocols. The same was done by four *nonexperts*, who knew little or nothing of a technical kind about the tests or how to interpret them. The case was also described by his wife and an acquaintance. Two clinical *experts*, who had access to X and to all the test results, also gave their diagnosis. In addition, X gave three self-appraisals: (*a*) *present* self, (*b*) *consistent* self (what X thought his personality had been, most consistently, over his lifetime), and (*c*) his *self-ideal*, that is, what he would like to be, regardless of how he thought of *a* and *b*. There were other, control, variables of some interest: for one, a *minimum amount* of information was given to two students who did not know X, namely, X's age, sex, marital status, occupation, and national origin. From this the students each offered a description of the likely personality of the subject so minimally indicated. A committee of other students also

deliberated and offered an account of what they thought the "typical" student was like, as a *cultural stereotypy*.

All these diagnoses, self-descriptions, and descriptions by others were conducted as Q-sorts, in terms of a suitable Q-sample of 120 trait statements. There were 46 Q-sorts, all about X. The 46 × 46 table of correlations was factored by Thurstone's technique, *simple structure* being achieved for correlated factors.

The design meets one of our requirements—it deals with a concrete situation in relation to X. Control variables are involved. For 46 variates of the above kind it is clear that the possibility exists of finding a factor solution which can at once represent many interrelationships between the variates and thus take cognizance of the obvious complexity at issue.

Pemberton concluded that seven primaries could be distinguished (for correlated factors). They were as follows:

1, 2, 3, 4. Factors for the diagnoses made in terms of the Rorschach, the TAT, the Sentence Completion, and the Standard Tests, respectively.

5. A factor for the *cultural role* stereotype.
6. A factor indicative of the temperamental differences among the various assessors rather than of X.
7. X's "essential" personality—the "best approximation of the consistent personality" of X.

Vernon (196) has already observed that the Thurstone search for *simple structure* is likely to result in the reappearance of the original variates as primaries,[1] and this is true in this case for factors 1, 2, 3, 4, and 5. Variate 6 is a reminder that assessors are likely to project something of themselves into Q-descriptions of others, which is a reasonable expectancy. Factor 7 alone appears to grasp X's personality. This factor was interpreted as an intuitive grasp that one may have about X: his acquaintance sees X this way, as do the clinical psychologists who interviewed X, and it is sometimes reached by experts from test protocols alone. X does not see himself this way. Nor does blind interpretation of the tests grasp this aspect of X in any compelling fashion—some of the experts hit upon it, others not. The factors 1, 2, 3, and 4 were reached almost as readily by some of

1. In the early days of the Spearman school, factors specific to such situations would have been expected: clearly, if four clinical experts analyze X's protocols for, say, the Rorschach test, it is highly likely that some variance common to all four will occur.

the *nonexperts* as by the *experts* themselves, and not all the latter were significantly loaded on the tests for which they were expert.

The analysis certainly covers something of the complexity which, as we have said, is to be anticipated. But we should expect a rather different set of relationships to be brought into view. It is somewhat difficult to leave the analysis with only one discovery—that an "essential" X exists. We anticipate more.

We reanalyzed the data, following our dependency lines, and our analysis of Pemberton's data therefore proceeds rather differently. The position we take, of course, is that a factor analysis should provide answers to propositions. We argued as follows: There is a reasonable expectancy that the two clinical experts who saw most of X, having interviewed him and having access to all the test results about him, should be able to provide an account of X that novices can scarcely hope to achieve or even to understand. We would also expect some clear evidence of dynamic relationships for a number of factors, all centered upon and pertinent to X. Thus certain expectancies are in one's mind before the analysis begins. These are then made explicit, as a few testable propositions, along the following lines:

Proposition 1: Since we can scarcely hope to explain the more simple by the more complex, it is reasonable to suppose that novices will have simple combinations, at most, of a few primaries, whereas the clinical expert's factor patterns should be complex, involving perhaps all the factors.

Proposition 2: X's variates, after analysis, should point to dynamic possibilities.

Proposition 3: The factor patterns for the experts who have only test protocols available to them, should be distinguishable from those for novices who try to grasp X's personality blindly from the same tests.

Proposition 4: We do not propose to be interested in factors which are specific to the various *tests:* that is, we expect specifics of the kind reached by Pemberton as factors 1, 2, 3, and 4, but we propose not to concern ourselves with them.

We then rotate the centroid factors with these propositions in mind, to see if there is a solution, for orthogonal factors, which satisfies all four. A solution could, in fact, be reached, using only four of Pemberton's centroid factors (the others relate to proposition 4 and are thus neglected). The four factors were then interpreted dynamically. It is difficult to describe these in language which conveys all that this involves, but the following will serve the purpose sufficiently well:

each factor, of course, may have positive or negative loadings, so we give the descriptions for both:

Factor A	(+)	Outwardly socially acceptable, tolerant, co-operative
	(−)	Not so
Factor B	(+)	Strong-willed, aggressive
	(−)	Submissive
Factor C	(+)	Relaxed, not nervous
	(−)	Tense, nervous disposition
Factor D	(+)	Inhibited
	(−)	Outwardly hearty, uninhibited

The Q-sample used by Pemberton was drawn from lists of generalized traits, whence the present language for the factors. Our own interpretation retains this language, but we regard these as surface descriptions, underneath which dynamic matters are at issue. That is, factors B, C, and D may represent either overt or covert matters; novices and inexpert persons may describe X overtly as, say, factor B, but the expert clinician would describe X, instead, as covertly so for the factor. X would not display certain aspects of himself for all about him to see openly but, clearly, has defenses which hide the real state of affairs. Again, whereas the inexpert may guess that X is likely to be nervous and tense (factor C), meaning by this that X is overtly so, the expert may mean instead that X is *really* nervous and tense (with all that this implies dynamically), but hides it from everyone around him, even from himself.

Thus we end with four factors, A, B, C, and D, so interpreted. Any variate may have any of these, positive or negative, in any combination, or none.[2] Proposition 1 means, therefore, that we expect the two experts, who saw X, to have most of these factors significantly, e.g., of the form:

$$A^+, B^-, C^-, D^+$$

2. For factors A, B, C, and D the number of possible categories of variates is very considerable, as we found earlier (pp. 107f.), when the notion of *simplest structure* was defined. Variates may be "pure" for a factor, i.e., significant in A, B, C, or D only; or may be "mixed," i.e., significant for AB, AC, . . . , or A, B, C, . . . , or A, B, C, D, . . . , with all possible combinations of positive or negative loadings. A factor solution thus becomes, in effect, a set of M propositions about the M variates at issue, the propositions being in relation to the factors. One can appreciate, therefore, how elegantly the factor method retains the complexity of a situation, yet has everything integrated and explained in terms of the few factors such as A, B, C, and D.

Novices and control subjects, on the other hand, are expected to provide, say, loadings on *none* of these factors, or on *one* only, or on a pattern diametrically opposed to that just given, i.e., they are expected to have patterns of the following order:

$$A^+ \text{ only },$$

or,

$$\text{Nothing significant}$$

or,

$$A^-, B^+, C^+, D^- \quad \text{(or the like)}.$$

Similarly with respect to proposition 2. To some extent it would be supported by a fact of the kind just mentioned—if experts have A^+, B^-, C^-, D^+, and novices have A^-, B^+, C^+, D^-, there is a presumption of a dynamic kind. But the proposition has reference more particularly to X's own self-descriptions: he is unlikely to be aware of B^-, C^-, and D^+, according to dynamic theory.

So we could continue. The analysis in no way ends, or is deliberated, in relation to any single factor which may be supposed to be X's "essential" personality. Rather, it is carried out with dynamic theory in mind, to see whether what is likely to be complex can be grasped in all its interrelationships and possibilities. The correct solution, if it can be reached, is one that brooks no contradictions for any of the 46 variates used by Pemberton, in any of the possible combinations of argument about them in dynamic terms. Such is what we have in mind as dependency analysis. An argumentative system is at issue, into which all the variates have to fit squarely.

We do not need to take the example further; a solution of the required kind was very readily reached. But we are also obliged to determine whether *other* solutions are as plausible as the one we accepted. By "other solutions" we would mean, again, a totally different argument, which covers all 46 variates and the complex relationships so indicated. It is quite a simple matter, for example, to place one factor through the two *experts*, and to end with a solution which allots only this one factor to them; some of the novices and inexpert subjects then have two or more factors, and our two experts only one. But the solution in this case, although acceptable enough statistically, does not make sense psychologically, and contradictions are involved as well. The investigator, however, is obliged to try out

such solutions and thus to support the sufficiency and necessity of the one that bears upon his propositions more directly.

DESIGNING SINGLE-CASE STUDIES

Studies by others, such as Fiedler (65), Bown (31), and Rogers (130), have been centered upon the therapeutic relationship in client-centered and other forms of therapy, using Q-technique and correlational theory as the operating tools. Fiedler studied whether therapists with divergent theoretical techniques (psychoanalytic, client-centered, and eclectic) also differed in their concept of an ideal therapeutic relationship, using Q-technique as the main operational technique. Fiedler also has demonstrated transference and countertransference along the same lines (65). In all such studies a Q-sample is employed, in terms of which a patient makes a self-description; but the therapist can also make one, using the same sample of statements; and either the client or the therapist can proffer a description of the other. Fiedler explores the relationships at issue, with suggestive results (65). An interesting study in this field is reported by Heine (79), who studied what, at the close of therapy, some patients felt about the source and outcome of clinical treatment to which they had been submitted. He compared a number of patients who had completed Adlerian, Freudian, and Rogerian forms of therapy or counseling, respectively.

Now when such divergent techniques (as in Fiedler's study) are being studied, or different attitudes (as in Heine's), resort has to be made to a sufficiently large number of cases, so it seems, to make generalization possible. Twenty cases treated by Rogerian, twenty by Alderian, and twenty by Freudian methods are the best that one can do as far as "large numbers" are concerned, but, certainly, this seems better than *one* case of each category. It will be said that one could scarcely draw any valid conclusions from a comparative study of *one* of each. *It is precisely against the assumption that this is impossible, however, that we have been directing all our attention in these many pages.* If the concrete conditions for each of three such situations are complex enough and if each can be represented by many variates, so that the data are complex, then valid conclusions are possible, in principle, from precisely such single cases. The facts can be reached in factor terms, much as we have discussed for Pemberton's data.

We may, of course, *repeat* our experiment on any *other* concrete situations. But, in principle, we might find that no two cases treated by the same therapy are ever fully alike, and yet provide clear-cut evidence that the modes of treatment have characteristic and *expected* consequences. Thus the supportive attitude of psychologist A in Nunnally's study, as against the nondirective of B, could find different expression in every case dealt with by the one therapeutic method or the other, and yet always be distinguishable in factor terms.

The use of "large numbers" is usually made feasible by *instrumentation* of some kind. Thus from self-psychological theory we may argue that a person who has had a wide range of interesting experiences in life, of a certain reality-testing kind, will be more stable, more open to successful counseling, or the like. One now makes an instrument, a test, consisting, perhaps, of a list of such possible experiences. A "large number" of cases (N) might be tested. The instrument would permit us to divide the N into two groups, those indicating many such experiences and those indicating few. The typical design would then, in effect, call for the validation of these results: the proposition might be asserted that those with few such experiences will be resistive to counseling or will volunteer for therapy or the like. We suspect instrumentation of this kind. It can be validated only in relation to differences between group means, for "large numbers" of cases. Sight is lost of the complexity of the matters at issue. The instrumentation is likely to beguile the investigator into assuming a concreteness and simplicity that really do not exist.

Nor is it difficult, usually, to redesign such instrumental (group-testing) studies so as to retain the more essential complexities, and yet to achieve their compelling and orderly restatement in factor terms. Thus Sears (141) has worked *instrumentally*, if we may use the term, in his studies of projection, "large numbers" of cases being tested. He had little success, and the facts, indeed, rather contradicted his anticipated results. We suggested elsewhere (174), however, that the problem should have been dealt with from the concrete standpoint of the single case, and an appropriate design for the purpose was outlined. Thus the mistake of asserting a general testable proposition about projection (as about anything else), leading to the use of large numbers of cases for impossible ends, finds its comple-

ment in the present conclusion, that the appropriate propositions can be asserted within the confines of the single case. This is not to say that there never is an occasion when large numbers are to be used—studies in child development, for example, may require normative procedures.

CONCLUSION

With this brief sortie into clinical psychology, we must conclude our introduction to Q-methodology and its applications to the study of behavior. Very much remains to be done, along both methodological and practical lines. In one way or another, however, Q-technique has applications in almost every nook and cranny of psychology in its research aspects. But this is not to suppose that we have a specific medicine for all the ills of clinical, or any other, psychology. Nor is Q-technique an open sesame to all manner of remarkable discoveries. The psychologist has to provide the theories; Q-methodology merely tests their consequences. But it does so in more than a merely statistical fashion, for, as Fisher (68) reminded us, it is impossible to separate the statistical from the formal matters of design, and impossible to separate these from one's theoretical preoccupations. Thus it is that the complex interrelations, to which attention has been drawn, are matched by the elegant subsumptive power of a factor pattern, such as we have referred to in many places in the above chapters as "simplest structure."

CHAPTER XV

REVIEW AND CONCLUSION

REVIEW

The task left unfinished before World War II is now reasonably complete. In 1935 we had put forward some very simple formulations about Q-technique, which, somehow, no one was prepared to take seriously. From the outset we expressed great confidence in these because we saw their applications everywhere, in regions hitherto untouched by testable propositions, including self-reflection (introspection), the moods, sentiments, and much else besides (171). All type-psychologists, we saw, could be supported methodologically. A single person could be made the subject of factor study. Why, then, were critics, whose judgment one must highly respect, so obdurate about the pros and cons of R and Q? The answer was to be found, we were always sure, in the different *systems* (171) at issue. For us, factor analysis was merely a tool, for use when we wanted to *test*, every now and then, some psychological hypotheses; we wrote, for example:

> I merely wish to provide a little inductive support, every now and then, for certain psychological hypotheses, and to this end I have reserved the right to distinguish between universes which I sample (and which provide me with error estimates), and variables (such as personalities), that I wish to manipulate *deductively*, and to which sampling conditions in no way apply [71, p. 196].

But others regarded factor analysis very differently and have continued to do so to this day. Burt's *Factors of the Mind* was oriented toward metaphysical matters: speculation about "atomic unity," Broadian "blobs," and the like were at issue for a purely theoretical component analysis, within the purview of *interdependency analysis*. We were not prepared to follow Burt into these Elysian fields. But this does not mean that we had no theoretical preoccupations of our own or that we did not feel the need for any. Ours would have to deal with psychological matters, and it was to these that we turned. In the practical realm the techniques of Spearman, Burt, Thurstone,

Thomson, Cattell, and their many followers were devices for massive field work, whereas ours was to be for the "clinic, the laboratory, or for rapid and subtle experimentation" (162, p. 18).

Meanwhile, every factorist of note seems to have regarded our standpoint as somehow unproved. Postulates and principles, however, are scarcely things one sets about proving in any formal sense; rather, we prove other things in their terms. The clarification brought about in these chapters, as far as Q is concerned, has resulted entirely from attention given to postulatory and protopostulatory matters. All else depends upon them. The principles subserving R and Q are different ones, and are irreconcilable. Basic to R, for example, is the use of *individual differences* to give substance to the transitory postulate, without which not a single correlation coefficient in that system could be acceptable for empirical regard. In Q the postulate is made to work, instead, by way of *intra-individual* "significances," a term we coined to cover at least one other way to make the transitory postulate "work." Nor did we know at the time that Keynes had used the self-same term "significance" before us, to refer to *any* principle mediating this postulate. Thus the principles of *individual differences* and of *intra-individual* "significance" are both subordinate to the more abstract principle of *significance* to which Keynes drew attention in his classical treatment of the correlation coefficient (94). It is upon such grounds that we have distinguished between R and Q.

Some of the protopostulatory undercurrents of R were referred to in chapter i: following Karl Pearson's example, the early factorists, and all R-factorists today, seek for verities by way of statistical inference only. We suspected any such program. Long ago J. M. Keynes was of a similar mind about the insufficiencies of statistical induction. Thus we forgo all considerations about "unitary" factors, of the kind attended to by our friend Sir Cyril Burt or by Sir Godfrey Thomson.

Nevertheless, the statistical tools used by factorists, it seemed to us, were admirably suited to serve an experimental methodology in psychology. The single concrete person, in James Ward's sense, could be studied in his own right in factor terms. There were two ways to support our scientific judgment about this. One was to look critically at the philosophy-of-science aspects of the matters at issue and the other was to undertake the experimental studies.

With respect to the philosophy of science there were two outcomes. One was support for a behaviorism of the widest scope. The other concerned scientific method itself, with respect to which we have no doubt been reiterative. But it seems to us that contemporary authorities, writing within the last twenty-five years, had missed a great deal in their discussions of scientific method. According to Cohen and Nagel (53), for example, science is almost wholly a matter of applied logic: truth, they tell us, is not to be found by the mere study of facts. They could be given the reminder that neither is logic alone ever likely to discover it. Ritchie (126), who was not a philosopher but wrote one of the best dissertations on scientific method, thought of it (as most scientists did until recently) as essentially a matter of inductive method, *plus* experiment *plus* the need to find proofs for propositions. Craik (54) followed Ritchie's lines, but his philosophical training led him to reject the crudities of *operationism* and to throw the baby out with the bath water by rejecting logical positivism as well. Even so, Craik argued for causative and dependency forms of analysis in science, against the puristic fashion for interdependency forms only, which in factor work stems from Karl Pearson and in physics from the modern theoretical physicists. We take our own stand with Kaufmann (92), who sees scientific method as deductive, but governed by empirical, that is, by specific inductive, procedures (which serve to solve problems). The methodology of propositions and postulatory principles with which we have expounded Q-technique follows Kaufmann very considerably, and, with him, Wittgenstein and Schlick and modern logical analysis. But there is missing in their formulations, if we are not mistaken, the postulate of the specificity of inferential interbehavior, which Kantor provides in his *Psychology and Logic*. We cannot pretend to have given a clear account of what this means. Yet this is the principle by which the scientist keeps his feet firmly planted on reality. Without it he loses himself playing a scientific game according to rules, which is what has happened to traditional factor analysis.

Kantor's principle lies behind the main thesis of these chapters, in a grass-roots manner. One way of conceiving science is to think of it in the grand Baconian fashion (which Pearson understood so well). Facts are gathered, and a theory induced to explain them. But, with more sophistication, theories can tell us *which facts are worth looking*

for. This, in a nutshell, is our philosophy of science. Thus the *K*-factors of a person's self-conceptions are facts to be discovered about him. They are set in a concrete situation; the scientist is selective in it, by way of his theories, his variates, and all else. The more penetrating the theory, the more profoundly interesting the facts are likely to become.

Otherwise, all the usual rules of scientific procedure apply to our experiments. We distinguish between synthetic and analytic propositions, between general and singular testable propositions, and between "general theoretic" propositions and singular testable ones. Our concern is with synthetic propositions which are either accepted or not on *empirical* grounds. These decisions are never irreversible—any proposition may be accepted pro tem, and be rejected later (the principle of *permanent control* of every accepted proposition [92]). No decision, however, must lead to incompatible conclusions. Nor can any synthetic proposition ever be excluded a priori—an injunction broken everywhere by the so-called "objective" psychologists who object to "subjective" data. All such matters are the basic, necessary creed of a scientist. There are many rules of procedure, too, such as that propositions should fulfil *predictions;* but prediction is not the be-all and end-all of scientific method. The *correctness* of a scientific decision is relative to the total scientific situation, that is, to the totality of propositions at the time—a rule that we used, if not to advantage, at least with some appositeness for exemplificatory purposes in earlier chapters. In addition to all these rules and elements of procedure, the method of centroid analysis, with its permissive solutions, retains for us the other principle, basic to all else, that scientific *inter-behavior* has its own specific contributions to make. Thus we exemplify again the pertinency of Kantor's basic principle.

Such, then, are the scientific principles upon which Q-methodology is based. With respect to *behaviorism*, the other consequence of our essays into the philosophy of science has been as salutory. Here again we start with the basic principle of Kantor, that the concern of psychologists is with behavioral segments. We have a very long way to go. But behavior is something concrete, with a beginning and an end for experimental purposes, albeit always rooted in never ending personal history. It seems to us that Burt, Cattell, Eysenck, Babington-

Smith, Loevinger, and other notable factorists failed to develop Q-methodology for themselves because they did not grasp the need for such concrete principles. Cattell has proliferated into P, T, O, and similar techniques. But these are actuarial in conception; time, for Cattell, just goes on and on, and one can, of course, take measurements at intervals across its path and count many happenings of one kind or another, as a statistician does who computes the vital statistics of deaths and births. Q-technique, instead, has applications in every branch of psychology where *behavior* is at issue, precisely because its concern is with *segments* of behavior, each a marked-off and separate event, to be studied in all its concreteness. Without this postulate, we, too, would be advocating ways of measuring persons for everything possible, but for no one knows what purpose.

Most important is the grasp of the external and internal "frames of reference" of behavioral segments. Subjective behavior is just such behavior. We have seen how to begin the process of bringing it to heel by determining lawful and coherent mechanisms at work in a person's Q-sorts. The bare beginnings have been made, and everything, indeed, remains to be studied. But the scientific lines seem clear enough.

The consequences of these methodological formulations have been many and far-reaching. We have seen how behavior of all kinds—attitudes, thinking, self-conceptions, personality, and social—can be experimentally studied in concrete settings, without constructing a single scale or measuring instrument of any kind based upon individual differences. Measurement is not quite the *sine qua non* of a developing science of behavior that American psychology has long supposed it to be. Nor are large numbers of persons necessary for our studies. In principle, one may work scientifically for a lifetime with a single case, X. What is essential is a good theory and faith that there are plenty more cases where X came from. The concern is not that of discovering something about a particular population or universe of statements as such in relation to a particular population of persons. We experiment, instead, with a particular person, or an interacting group of them, who operate with samples of Q-statements. Our generalizations have reference to the *scientific situation* of an experimentalist, and statistical method is just one component of this

situation. Our general conclusions are not merely statistical inferences, nor have they any need to be. These, then, are the broad principles upon which Q-methodology has been laid.

THE NEWNESS OF Q

The question is frequently raised about the "newness" of Q-methodology. It is usually said that, after all, correlating *persons* is nothing new but dates back to the first decade of this century. Q is a methodology, dependent, it is true, upon correlation technique, but it is as new as a freshly built house, even if it is built with old bricks. Q commands two main statistical and experimental procedures. One consists of representing theories, as explanations, in variance designs; the explanations are examined by Fisher's methods of variance analysis and small-sample doctrine. Nothing could be neater, in this respect, than the way we represent a theory as a matter for *use*, and not for direct proof for any supposed general implications. A theory is a growing point for experiments, to help the investigator find his way about in reality, as Schlick put it. So, precisely, is a structured Q-sample. The other procedures available to us are those of factor analysis. These serve to put singular propositions to test, that is, to solve our problems. But we replace the old notions of factor analysis as an *interdependency* matter by a frank recognition of its *dependency* possibilities only. Thus factor analysis is used to test propositions, and variance analysis to explain them (and to offer some proof of the explanations).

The separation of R and Q, it seems to us, is essential. We have dealt somewhat drastically with R and the psychometry it subserves. This is not to deny its great technological importance—mental tests, after all, are useful in the workaday world. But R is very much a punctured tire. It is not the great scientific methodology that its sponsors have believed it to be. Its great error was to regard theories and general laws as equivalent, both testable by way of general propositions; thus it became a technology of individual differences, divorced from psychological theory. It is obviously true, however, that man has a multitude of *characteristics*. So has a chair—it is made of wood, is so high, so tensile, so heavy, and so forth. R deals with such listings of all man's characteristics, the assemblage of his attributes, and seeks to reduce these to an atomic-like order. What is

achieved this way could, in principle, be important, because factors in R represent man's *potentialities* and *capacities*.

In Q, on the other hand, the concern is with concrete behavior and the single case. The concern is with how a particular chair, so to speak, fits into a particular room. Some characteristics of all chairs will no doubt be important, but others not: a chair for a bedroom is one thing, and for a public library another. No doubt our particular chair should have somewhere upon which one may *sit*—yet even this is not generally true, to judge by modern design, where some chairs are made to *lie* in, with disaster ahead if one tries, instead, to sit down. Similarly for human behavior. What actually occurs, and not *generalities* or potentialities, must engage our attention. Nor is it correct to suppose that one could not study this concrete behavior, fundamentally, without specifying potentialities of some kind. Rather, it is more correct to suppose that one infers or has theories about potentialities (if at all) as the outcome of inquiries into concrete behavior. Clarification of such issues is surely important and a sign of new things ahead.

The newness of Q is shown also by its pertinency to the single case, and we are only at the bare beginnings of our applications in this direction. That we are on the right lines would seem to follow from a recent plea by Babington-Smith (12), which at the same time provides us with a veritable coup de grâce. In remarks about the difficulty of accepting Q-technique as "new" Babington has this to say:

> I think it would indeed be something new if Stephenson has found a way of overcoming [the] recurrent difficulty about personality ratings, as to whether ratings are of what a person is, or what he would rate himself, or what his friends would rate him, or even his enemies [p. 82].

These matters, of course, are the meat and substance of Q-technique. We deal with precisely these conditions of instruction almost every day. But we should ask, surely, why there should be "recurrent difficulties" of this kind, as to whether a person is what he thinks he is, or what his friends think of him, or his enemies. They occur, such difficulties, only for those who have categorical propositions in mind, in a protopostulatory manner, that is, where there is a belief that something "real" or "true" can be grasped, if only the "difficulties"

can be obviated or dissolved. It was against precisely such categorical matters that Q-technique was directed in the first place.

With respect to factor analysis and variance design we have had much to give, too. Fisher's methods served experimental method, whereas traditional factor analysis did not do so. The end of a factor analysis was always an interpretation, explanation, or hypothesis about specific facts which had been reached by way of statistical analysis. The end of an experiment is always a statement of confirmation, or not, for a previously asserted proposition. Theory comes first in the one case, and last in the other. We reverse the traditional roles of factor method and variance design, yet retain theory in its prior position for both. Factor analysis becomes an experimental procedure, and variance design becomes a vehicle for explanations and theory. There can be no doubt that the structuring of samples is something essentially new.

PSYCHOLOGICAL OBJECTIVES

It has long been the sentiment of Englishmen to hope for better things—*spero meliora*—and it seems fitting to express the same aspiration with respect to a final question that we propose to raise, namely: What kind of psychology does all this methodology seek to subserve? We dare to ask the question and to answer it, because of the widespread ramifications of the methodology about which perhaps more than enough has just been written. There are psychologists who have little faith in expositions of *method*, regarding these as hopes and good intentions, and who believe that an important and detailed experimental study of some kind would be more to the point.

Yet important matters are surely at issue. During the past decade there has occurred a most interesting change of scene in the great play of psychology. From three widely different sources the study of *subjectivity*, if not *consciousness*, has been restored to the stage. These come, first, from American self-psychology; second, from Soviet psychology; and, third, from logical analysis and the study of behavioral segments as defined in these chapters—which, if it has to be given a name, could best be called "behavioral science." Let us end our dissertation by looking at these three sources of modern concern with man from his own subjective standpoint.

It is difficult to believe that consciousness does not really exist within the human mind, as its most obvious attribute and function. Clearly, all depends upon what is meant by "consciousness" as distinct from "subjectivity." We believe that there is no *substantial* consciousness for a behavioral science to be concerned with; yet it is possible to explore subjectivity, as never before, along Q-lines. Subjectivity is behavior; consciousness as ordinarily regarded is something else. This is our standpoint.

It will be said that, surely, this is merely to quibble about terms. Nevertheless, some caution is indicated. Two widely separated sources of modern psychology, quite independently it would seem, have made claims to have restored the study of consciousness to the psychological scene. The one is American self-psychology; the other, official Soviet psychology. The former, we have argued, belongs to our representative probes a(i) and a(iii), as does all phenomenology. In so far as it has led to more serious concern with man's subjective behavior, the advance is considerable, and we support it. But its main concern, the *self*, is a matter of operations of the Q-kind, for probe a(ii), which notable self-psychologists believed to be a region of private concern to a person and incapable, therefore, of direct testable operations. Moreover, any behavioral segment can be examined by way of all six of our representative probes, internal as well as external, so-called. This matter seems to the writer to be inescapable and provides a warning, at least, for some caution about excessive claims for any particular psychological system.

Soviet psychology represents a rapidly changing scene, too. Apparently materialism is being replaced by a return to the study of psyche (*2*). The fundamental doctrine, it appears, is a dialectical law concerning quality-quantity changes. A process of a quantifiable kind may proceed up to a certain point, such as, for example, may concern chemical changes in a nerve. But a new "synthesis" is supposed to take over at that point, and a new quantitative principle results. What was chemical becomes electrical, with its own distinct quantitative laws. What was physical becomes physiological, and what was physiological becomes mental. Each such qualitatively distinct *system*, as the Soviet psychologist would say, has its own laws, and the laws of phenomena at one level cannot be expressed in terms of laws at a lower level. Thus *reductionism* of all science to phys-

ical operations is rejected. Consciousness, clearly, is given a rationale; it is considered to be a new synthesis of matter, irreducible to the laws of physiology. Along these lines it is held that the study of animal behavior and of physiological psychology is unlikely to make contributions of any significance to the science of consciousness.

Consciousness is thus returned to psychology, even in Russia, home of Pavlov and Bechterev, twin pillars of physiological behaviorism. As in the case of American self-psychology, this probably represents a scientific advance. One may marvel, indeed, at the concomitance of emphasis from two such widely different sources as America and the Soviets. Both, there can be little doubt, are a reaction to the narrow conception of behaviorism which has characterized the thinking of psychologists in the two countries for several decades.

Our own standpoint contemplates no such metaphysics as the Russian. Instead, we believe that all that need be at issue is behavior, whether this is subjective to a person or objective to others. If the reader prefers it, behavioral science is so restricted. Anything subjective that the self-psychologists or the modern Soviet psychologists care to explore scientifically can come within the purview of our representative probes, all, that is, except *a*(i) (p. 96). We wish to retain behaviorism as the science of behavior.

Behaviorism itself began as a methodological issue and not as the study of any particular problem in psychology, a usurpation that conditioning theory has imposed, with, we believe, some baneful consequences. It cannot be denied, however, that nowadays American psychology, such as is based upon behaviorism, is dealing concretely with learning, motivation, and the like with some measure of success. With Hull and his brilliant cohorts, psychology has almost become a Newtonian-like science, with a few postulates and definitions, a single fundamental law or two, and all else a discipline of the hypothetico-deductive method. The basic problems are found in animal learning, and the inquiries into this bid fair to monopolize much effort for years to come. We merely suspect this narrowing of behaviorism of a certain misanthropy. In this we are perhaps a reflection of the Irishwoman's remark, as she watched a military parade, that her son alone was in step and all others out. But there are others in the contemporary scene who seem to be reaching out into more

pressing, if not more profound or basic, problems of mankind. They are the clinical and social psychologists of the United States. Our own stand must be with any such who take a behavioral segment for their study. Historians, novelists, biographers, philosophers, social theorists, and playwrights, when they are concerned with human behavior, are within our purview. Recently a week-long conference was held in Oxford, at which many of the leading scientists and scholars of Great Britain participated, to discuss such matters as the "creative mind," man's "liberties," "social progress," "industrial relations," and the like. In a list of 100 speakers, only one was a professional psychologist. It is precisely such topics that we believe to be the proper concern of an experimental methodology such as we have been outlining. But little about these matters has been reduced, up to now, to testable operations. We can now see how to do this. If, in the pursuit of them, we can gain insights about man, such as a Freud has grasped or as a Tennessee Williams has, with the agony of hurt of a Blanche du Bois, then all our petty statistical and postulatory procedures would be repaid a thousand fold. Our standpoint is that we have now, as never before, some probing possibilities into such regions of study.

But there can be no mentalistic science, such as phenomenology made a pretense of subjugating. Ours is the kind of content that the biographers have freely managed to use all down the centuries—we seek to study men's motives, their sayings, musings, imaginings, doings, thoughts, reveries, dreams, cogitations, jealousies, and all else of the kind that a Dickens or a Shakespeare or a Hawthorne dwelt upon. This is the region of *subjectivity*, and perhaps of *personality* as such. It is the first of the main areas of concern for a future psychology. But man's *social behavior* is as significant. We have merely advanced a methodology for these same regions of study, consisting of the postulatory-dependency method of these several chapters, where the complexity of man's behavior is matched by the need for some form of multivariate analysis of experimental situations.

We are slowly emerging from distrust of theory in psychology. There is evidence that new methods exist for probing into man's subjective behavior. For these, theory is essential, to help us discover what is observable in principle. The methodology we have tried to outline offers some tools for any such theory.

How, then, are we to bring the long argument of these chapters to a conclusion, to clinch it, if possible? A mathematical proof is scarcely possible for postulations of a methodology. A remarkable discovery, using Q-technique, could have spoken for itself. The voice is not altogether mute. All that we can offer seriously, however, is a realignment of much that has passed for psychology in the last hundred years, as well as a grasp of experimental possibilities not open to us before. Contentious issues have been considerably softened or resolved. The objectivity of "subjectivity," the claims of nomothetic versus idiographic principles, the definition of samples, proper regard of psychological *types*, the status of the "single case," and much else of the kind have passed in review. Much is no doubt still unclear or enigmatic. We believe, however, that a methodology has been provided upon which a great deal indeed of behavioral science can be based. With this we rest our own single case.

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